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THE ELECTRICAL RESPONSES OF THE EAR

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The recent enlargement of experimental resources and techniques, and particularly the development of vacuum-tube apparatus for the amplification and recording of electrical potentials, has resulted in a broad extension of the field of sensory research. This review deals with recent work in which electrical methods have been employed in the investigation of the ear.

Electrical potentials as a result of stimulation of the ear by sound may be observed at various levels of the acoustic system: in the cochlea, in the auditory nerve, and in successive tracts of the central nervous system. It is the part of auditory research in this field to trace out the whole chain of events initiated by the entrance of sound waves into the ear and terminated by auditory perception and response.

A comprehensive review of this field has not previously been made, though there have been a number of treatments of restricted scope. Discussions of a limited character have been made by Caussé (14), Davis (28), Hallpike (73), Piéron (117), Reboul (126), Ruckmick (128), and Stevens and Davis (135). There have also appeared several summaries of the work of particular laboratories (Crowe and others, 18, 90, 91; Davis and others, 29, 36, 45, 134, 135) as well as discussions of the character and theoretical implications of the phenomena (Hallpike and Rawdon-Smith, 77, 79; Kreezer, 104, 105; Wever, 141, 143; Wever and Bray, 146, 156).

I. History

The history of this subject begins with early demonstrations of the electrical activity of the brain. Here Caton (13) was the pioneer not only in noting the presence of such activity in the cerebral cortex of animals, but also in showing that the activity is altered by sensory stimulation. He seems to have used visual stimuli only, and it

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remained for Beck (9) and Danilewsky (27) to show, several years later, that sounds are likewise effective in modifying the activity of the cortex. These studies, like the brain potential studies of more recent times, were concerned with the problem of cerebral activity, and not specifically with the working of the sensory system. They showed that sense-organ activity is reflected in electrical potentials, but their relation to further developments is incidental.

Piper (118–120) in 1906 made the first deliberate attempt to study the operation of the acoustic system by electrical methods. His problem was the function of the labyrinth in the fish, and he sought a solution by placing electrodes on the exposed otolith of the saccule and leading the currents to a galvanometer. Deflections were observed on stimulation under water with pipes of 100 and 260 vibrations per second and on striking the tank containing the preparation.

The first study of the electrical responses of the ear in mammals was that of Buytendijk (12) in 1910. He exposed the auditory nerve in rabbits and guinea pigs, applied electrodes which were led to a galvanometer, and observed deflections on stimulation with intense sounds of various kinds, such as a pistol shot and the note of a flute.

Forbes, Miller, and O'Connor (54) in 1927 looked to electric responses for an answer to the problem of the representation of frequency in the action of the auditory system. They placed electrodes on the brain stem or medulla of decerebrate cats, and with a string galvanometer recorded the impulses resulting from sound stimulation. Sharp noises, like the clicks of a watchman's rattle, gave impulses that were coincident with the stimuli. Continuous sounds, like the tone of a tuning fork, however, gave unsatisfactory results. A 104∼-fork produced responses that appeared 'imperfectly' synchronized with the sound waves, and a 200∼-fork seemed sometimes to do likewise. A card held against a cogwheel gave impulses up to about 220 per second. The authors concluded that the synchronization of nerve impulses and sound waves was more apparent than real, and was actually due to a piling up of impulses at the points of maximum amplitude of the waves.

In 1930 Foà and Peroni (53) worked on the giant sea turtle, and with a string galvanometer recorded impulses from the facial-auditory nerve (which is a single trunk in this animal) during acoustic stimulation. They observed discharges of 50–60 per second, irrespective of the character of the stimulus. In a later experiment

on a different species of turtle, Wever and Bray (**148**) were unable to reproduce their results, and it is probable that the impulses were facial rather than auditory.

In all of the early studies there were severe limitations of method. The direct-current galvanometer is capable of revealing only slow alterations of potential. The string galvanometer records more rapid changes, especially if the string tension is raised, as in the experiment of Forbes, Miller, and O'Connor, and the resulting loss of sensitivity is compensated for by amplification. However, under the best conditions the string galvanometer is suitable only for low-frequency responses; in the experiment of Forbes, Miller, and O'Connor it became practically inoperative a little above 200∼, though a somewhat higher limit is possible (**125**).

The development of vacuum-tube amplifiers for the faithful amplification of minute potentials has brought about a recent wave of activity in the study of the relations of the auditory system to its stimuli. In 1930 Wever and Bray (**144–147**) reported experiments in which an electrode was placed on the auditory nerve of the cat, with a grounded electrode elsewhere on the animal, and the currents led through a vacuum-tube amplifier to a telephone receiver. Responses were observed which were synchronized with the waves of sound over a wide range of frequencies. Extensive checks were made to exclude artifacts, and the phenomena were shown to depend upon the physiological activity of the auditory system.

These results have since been verified and extended by numerous investigators. The potentials generated in the cochlea have been distinguished and, in some degree, separated from those of the nerve itself, and limited observation has been made of responses at higher levels of the nervous system.

II. APPARATUS AND TECHNIQUE

Apparatus

The apparatus needed for experimentation in this field includes sources of sound, amplifiers, and recording equipment. The particular arrangements vary with problems and methods; some of these have been described by Wever and Bray (**147, 155**), Witting (**167**), Garceau and Davis (**60**), and Hughson, Thompson, and Witting (**93**).

1. *Sources of Sound.* For exploratory study, various simple sources of sound have been employed, but exact work requires an audio-oscillator operating into a loud-speaker. This apparatus should have a wide range of frequency and intensity, and should

include suitable attenuators and filters for control of intensity and wave form. A determination of the absolute intensity of the sound stimuli is highly desirable, and may be made with a calibrated microphone.

2. *Amplifiers.* The gain and other characteristics of the amplifier will depend upon the method and equipment used in recording. Generally speaking, the amplifier should have the greatest gain and fidelity possible, and a wide range of frequency and intensity together with a minimum of background noise.

3. *Recording Apparatus.* The most generally satisfactory instrument for recording is the cathode-ray oscillograph. It has no troublesome frequency characteristics and withstands overload without harm. Its single drawback, low sensitivity, can be compensated for by high amplification. Observations can be made visually or photographically, and may include wave form as well as amplitude. Other measuring instruments, like the vacuum-tube voltmeter, give no indication of wave form, except indirectly when incorporated with additional apparatus for harmonic analysis.

Technical Procedures

1. *Anesthesia.* All the common anesthetics have been used, and none has shown any marked effect upon the cochlear response (12, 73, 130). Deep anesthesia, as from ether and chloroform, reduces the nerve potentials, especially those from higher tracts (129). Decerebration, with time for recovery from anesthesia and shock, gives most satisfactory nerve potentials.

2. *Operative Procedures.* The modes of surgical treatment vary according to the problem, but 2 approaches have proved particularly useful. These are the eighth-nerve approach and the middle-ear approach, respectively described for the cat by Wever and Bray (147) and by Crowe and Hughson (18, 90).

3. *Electrodes.* The active electrode varies greatly in type and location. For the recording of impulses from the round window, cotton-wick and silver-foil electrodes are especially valuable. For recording from the nerve, a fine wire insulated except at the tip and the Adrian-Bronk hypodermic-needle electrode have been extensively employed (130). In the last type, the outer casing is commonly used as the grounded electrode; in the other types, a silver plate or other large conductor is placed in contact with some inactive tissue of head or neck. Nonpolarizable electrodes have been tried, but have no obvious advantages.

4. *Methods of Observation.* The methods of observation largely depend upon the recording equipment. In the early studies of Wever and Bray the amplifier was operated into a telephone receiver, and the method was one of simple listening. Nearly all investigators have followed this procedure for exploratory work.

The listening method has obvious characteristics that arise from the incorporation of the observer's ear in the system. It is arduous, slow, and inaccurate; yet not all of its characteristics are undesirable: high sensitivity can be achieved within the range of frequency in which the human ear is acute, and the analyzing power of the ear is an asset in the presence of a large background of extraneous potentials. For rough quantitative measurements the method has taken the following forms: (a) determination of the least intensity of stimulus to give a just-perceptible response; (b) determination, for a constant stimulus, of the amount of attenuation necessary to give a just-perceptible response; and (c) evaluation of the strength of the response by equating it to some external standard. Methods (a) and (b) are equivalent only if the responses are a linear function of stimulus intensity, which usually is not the case. Method (c) is likely to yield results that are inconsistent and misleading if strong stimuli are used and the responses fall in the region of overloading (160).

With oscillographs and meters a visual mode of observation is substituted for the auditory, and provides the advantages of ease, rapidity, and precision. For general exploration the auditory methods have their uses, but for quantitative work and studies in which the magnitude of responses is changing rapidly the visual methods are obviously preferable.

5. *Precautions.* In this type of experiment, many precautions are necessary to avoid the introduction of artifacts. The primary dangers are electrical induction and microphonics, which may be avoided by suitable electrical and acoustic shielding (147). The efficacy of these measures may be checked by the use of artificial preparations, tissue-killing reagents, and the like.

It may be mentioned that 2 electrodes in a vessel of water or salt solution, or the more elaborate device of Gatty and Rawdon-Smith (61) made of frog's skin in a tube of salt solution, will yield electrical potentials when exposed to intense sounds (see also Kupfer, 107). Likewise, potentials are generated by moist tissues anywhere on the animal. The distinction of these spurious effects from auditory responses is made initially in terms of their low magnitudes in relation to the magnitudes of the stimulus, and is further

clarified by their functional relations to the stimulus and their reactions to anesthesia, drugs, blood supply, death, location of electrodes, and surgical procedures (3, 77, 147).

It cannot be too strongly emphasized that in this type of investigation great care and vigilance are necessary to guard against the entrance of error. There is little doubt that some of the discrepancies of experimental results which appear to perplex us are due to inadequate precautions.

III. General Characteristics

The early work in this field revealed the physiological nature of the responses, and brought out the distinctive characteristics of the effects from the cochlea and from various elements of the acoustic nervous system.

The physiological nature of the responses was shown in a number of experiments (Wever and Bray, 147). The responses decrease in intensity and finally fail on restriction of the blood supply and on death of the animal. A direct current applied to the tissues reversibly reduces or eliminates the responses, either through polarization effects or electrolysis. The responses are dependent upon the integrity of the auditory system, and disappear when the cochlea is destroyed or when the electrodes are shifted to other tissues remote from the cochlea.

The responses of the cochlea and of the auditory nerve differ in a number of respects, as has been brought out especially in the work of Adrian (1, 2), of Adrian, Bronk, and Phillips (3), and of Davis and Saul (40, 129–131).

1. *Localization.* The cochlear response has its focus of intensity at the cochlea itself and is most effectively picked up with the electrode on the round-window membrane. The nerve response is obtained from the nerve trunk, and, under conditions of low intensity of stimulation and moderate amplification, a movement of the electrode barely off contact with the trunk causes a significant drop or a loss of response. The responses in the central nervous tissues are restricted to the acoustic tracts.

2. *Anesthesia.* General anesthesia seriously affects the potentials from the central nervous system, but does not appreciably impair the cochlear responses.

3. *Blood Supply.* The nervous responses fail more rapidly than the cochlear responses after interruption of the blood supply to the head and after the death of the animal.

4. *Wave Form.* The cochlear responses reproduce the wave form of the stimulus with considerable fidelity. The nervous responses do so less exactly.

5. *Magnitude.* Under some conditions the nervous responses are stronger than the cochlear potentials. At high levels of stimulation, however, the cochlear responses are the stronger. The nerve responses have thresholds, while there is no indication of them for the cochlear potentials (**161, 165**).

6. *Frequency Limits.* Cochlear responses may be observed to tones over a great range of frequencies, which varies with the species of animal studied (see Section IX). Nervous responses fall off rapidly in intensity as the frequency is raised, and under most conditions of observation they show a limit much below that of the cochlear response.

The above features distinguish 2 types of response, one in the sense organ, and the other at various levels of the acoustic nervous system. The cochlear responses persist after interruption of the central nervous connections, provided that the cochlear blood supply is maintained (**69**). On the other hand, the nervous responses are dependent upon the cochlea and cease after its destruction (**145**).

The relatively rapid disappearance of the nerve responses after interruption of the blood supply is probably not due to asphyxia of the nerve, but is an indirect effect which arises from the dependence of nervous upon cochlear activity. As the end-organ is impaired, both cochlear and nervous potentials decline, but the latter, which under most conditions of recording are relatively weak, are the first to disappear.

IV. THE COCHLEAR RESPONSE

The characteristics of the cochlear response vary somewhat with the species of animal concerned. The following discussion will deal principally with this response in the cat, an animal which has been extensively studied and whose ear represents a relatively high degree of development.

1. *Relation to Intensity.* For a tone of a given frequency, the cochlear response changes in a definite manner as a function of intensity. The form of the function has been studied by several investigators. According to the results of Wever and Bray (**155, 161**), the most general form is that of a power function, in which the electrical response is proportional to the sound pressure raised to a power that usually is a little below unity. Typically, high tones yield

functions of somewhat smaller slope than intermediate tones, but individual differences are common. Covell and Black (16) obtained functions in which the slope (*i.e.* the power of the function) was unity, or nearly so. Davis and his collaborators have described the function variously, but in their most recent report (135) they stated that the 'ideal' form is linear, but that this form is usually departed from at low intensities. Wever and Bray (161) have presented evidence in support of their position that departures from a simple function at low intensities are probably due to extraneous potentials.

At high intensities the responses depart from the form described, as overloading appears (95). The curves approach a maximum and bend downward as the stimulus intensity is raised. A further increase in stimulus intensity greatly diminishes the responses, until ultimately the ear is damaged.

If the cochlear response is taken as indicative of the essential functioning of the receptor, as most investigators now agree, the form of the intensity function reveals that within its normal range of operation the ear is a sound transducer of great fidelity and efficiency. For intensities of great magnitude the efficiency falls, at first slowly, and then very rapidly. The form of the function at high intensities evidently means that central elements of the acoustic system are somewhat protected from excessive stimulation.

The maximum value of the cochlear response varies in a systematic manner for different frequencies. For the cat, the highest values of the maximum have been obtained for tones of the middle range (16, 161). However, low tones have not been thoroughly studied in the cat. In the guinea pig they give very large potentials (165).

Under normal conditions, the intensity functions manifest high constancy: repeated measurements agree. This fact has led some investigators to use the maximum of the curves as a point of reference. Thus Davis and his associates (36) have often expressed their results in per cent of the maximum for a particular stimulus, and Covell and Black (17) have particularly recommended the use of the maximum for comparative purposes. However, it should be borne in mind that no single point on the intensity curve will serve as a substitute for the complete function. Responses at high levels sometimes behave differently in the face of adverse treatment from those at low levels. For example, in an experiment on the effects of the application of sodium chloride to the round window, Wever and Bray (159), under certain conditions, observed a reduction of responses that was progressively greater at higher levels of response: the intensity function suffered a general reduction in slope.

2. *Relation to Frequency.* Responses have been obtained to tones from 35~ up to 30,000~ (158). Cats have not been tested below 35~, but guinea pigs give responses to sinusoidal waves as low as 5~ (165).

A curve which expresses for a number of frequencies the sound intensity required to produce a response of a given magnitude is called an equal-response curve, and has been regarded as indicative (in an inverse manner) of the frequency sensitivity of the peripheral system. Equal-response curves vary in form for different animal species (158), and to a lesser degree for individual animals of a given species. They vary also with the level of response, by virtue of the fact mentioned above that the slopes of the intensity functions vary for different frequencies. At a relatively low level of response, a typical cat shows the highest sensitivity in the middle range of tones, from perhaps 500~ to 7000~, and the sensitivity falls off rapidly for lower and for higher tones. At high levels of response the curves for the cat usually show a particularly rapid loss of sensitivity for the lower tones (161).

A family of equal-response curves bears a resemblance to the curves of equal loudness for man. Stevens and Davis (134) have taken the position that " it is the behavior of the receptor mechanism which imposes the form which we find in equal loudness contours." Their conclusion was reached after a comparison of a selected loudness contour and what they termed 'thresholds' of cochlear responses for the guinea pig. Their 'thresholds' were minimum perceptible deflections on the cathode-ray oscillograph, and it has been pointed out (Wever and Bray, 161) that their observations were as much a function of their apparatus and conditions as of the ear of the animal, and that the results for different frequencies were not comparable. Moreover, cats and other animals give different types of equal-response curves (158).

There is no question that if the cochlear response reflects the actions of the receptor its functions are ultimately related to those of loudness, but the intervention of central processes between the receptor and perception reduces the likelihood that this relation is a simple one.

3. *Wave Form.* For sounds of moderate intensity, the wave form of the cochlear response reproduces that of the stimulus (1, 125, 144). This relation probably holds generally, though observations at the lower frequencies of stimulation are complicated by the admixture of nerve impulses (28, 34). At extremely low frequencies the nerve spikes become of relatively short duration in the cycle, and

thus are easily distinguished (165). In the guinea pig, reasonably good reproductions of the wave form of the stimulus continue as low as 15~ in some individuals, provided the intensity is only moderate. Lower tones give responses which are greatly distorted for all intensities at which they have been observed.

The responses to all tones show distortion at high levels of intensity, and depart from a sinusoidal wave form (137, 162). An analysis of the waves reveals the entrance of numerous harmonics, often as high as the sixteenth for a 1000~ stimulus. At relatively low levels of response the partials have magnitudes which fall off in numerical order, but generally the odd partials have steeper slopes and at high levels outstrip their neighboring even partials. Thus, for 1000~ the third partial has a higher maximum than the second, and the fifth a higher one than the fourth. Sometimes a. similar relation can be made out for the sixth and seventh and higher partials, but more often the magnitude of these is so low that comparison is difficult.

The increase in harmonic content with increase in intensity follows fairly closely the form of the intensity function in its departure from uniformity of slope. In one experiment (162) the total harmonic content rose from 10% at the first marked deviation from uniform slope to over 30% at the maximum of the curve, and finally reached 50% as the curve bent downward. Yet stronger stimuli would doubtless have revealed more severe distortion, as indicated by the waves obtained for very low tones, though these have not been analyzed (165).

A study of the results of wave analysis shows that the change of slope in the intensity function represents a true loss of efficiency and is not simply a transformation of energy into higher frequencies, since a summation of the responses for all partials gives the same form of curve as the fundamental alone (162).

If 2 tones are simultaneously presented, each produces its series of harmonics, and in addition there appear numerous interaction products in the form of combination tones. Newman, Stevens, and Davis (116) observed as many as 66 tones as the result of stimulating with 700 and 1200~ at high intensity. Also, the introduction of a new tone modifies the harmonic pattern of a tone already present.

The results on distortion indicate that the ear of the cat is both nonlinear and asymmetrical in its response to impressed vibratory forces. The mathematical expression of its action involves a

Fourier's series in which the necessary number of terms increases rapidly as the stimulus intensity is raised.

Stevens and Newman (137) cited evidence which led them to the conclusion that the muscles of the middle ear play a major rôle in the production of distortion. Deep anesthesia, which they believed to result in depression of tonus of the tensor tympani muscle, greatly increased the even harmonics, but failed to affect the odd. The results of Wever and Bray (162) do not agree with the conclusions regarding the action of the tensor tympani muscle. Denervation of this muscle and cutting its tendon produced no striking alterations in the pattern of distortion. Elicitation of the tensor reflex by stimulation of the pinna and the application of artificial tension to the tensor tendon produced a reduction of all components, with no characteristic difference in the effects upon odd and even partials. These results were interpreted to mean that tension of the tensor tympani muscle reduces transmission without otherwise seriously altering the mechanical state of the ossicular system. The behavior of the stapedius muscle in this relation is as yet obscure.

Distortion in the ears of animals as revealed by the cochlear response has been compared with distortion in man (116), and such a comparison substantiates the view that distortion is primarily, at least, a peripheral affair.

4. *Fatigue.* A depression of electrical responses as a result of stimulation of the ear was first shown in experiments of Davis and others (35, 36) and of Hughson and others (91, 138). A thorough investigation of the effect was made by Hughson and Witting (95). They found that after stimulation with a tone of extreme intensity the responses to all tones were impaired. Under their conditions the effect usually lasted for about 24 hours, and recovery occurred after about 72 hours. The effect was more marked when the muscles of the middle ear were divided. On the other hand, the effect was diminished or absent when artificial tension was exerted upon the middle-ear muscles, or when some other procedure reduced the efficiency of sound transmission. It was suggested, therefore, that the site of impairment was the sensory cells of the inner ear. There was no obvious damage to the organ of Corti, however (93).

Hughson and his associates worked with the electrode on the auditory nerve, and probably recorded both cochlear and nervous potentials. There is good evidence that both these types of response suffer 'fatigue' effects, but in the above results it is not possible to assign to each type its proper share.

Stevens and Davis (134) obtained essentially similar results from the round window, and their observations bear out the suggestion that the primary effects are located in the cochlea. They called the effect 'hysteresis.'

Wever, Bray, and Willey obtained impairments like those described only with low tones for which intensities up to 1000 dynes per sq. cm. were available (165). Other tones failed to show any reductions after many minutes of stimulation at intensities that gave maximal responses (unpublished observations). Hence it is likely that the other experimenters mentioned made use of intensities somewhat greater than those available to Wever, Bray, and Willey; it is unfortunate that they were unable to state the absolute intensities required for these impairments.

In view of the extreme intensities required, and the long period taken for recovery, it would seem more appropriate to regard the effect as one of injury rather than as fatigue in the usual sense. It is probable that stimulation deafness (see Section XII) is a later and irreversible stage of the same injury process.

V. Origin of the Cochlear Response

The physiological character of the cochlear response and its dependence upon the acoustic apparatus are unquestioned. Its specific point of origin within the cochlea and its relation to the process of nerve excitation, however, form matters of issue. Three theories regarding the origin of the potentials are: (1) the membrane theory, (2) the nerve theory, and (3) the hair-cell theory.

1. *The Membrane Theory.* Adrian (2) first suggested that the movement of fluids or membranes within the cochlea might give rise to the cochlear potentials. He gave up the hypothesis after he found a weakening of the responses as a result of cooling, and their failure on cocainization and death (Adrian, Bronk, and Phillips, 3). Bast and Eyster (7) suggested a fluid pressure across some membrane as a possible source of the effects, and Leiri (108, 109) discussed such a hypothesis.

Hallpike and Rawdon-Smith (77, 79) have been the chief exponents of this type of theory. They suggested that the potentials are generated in the movements of fluids separated by a membrane, and pointed to the endolymph and perilymph, separated by Reissner's membrane, as being likely to fulfill the requirements. As these fluids are of different origin, they may be of different ionic concentrations, and thus may polarize the membrane.

2. *The Nerve Theory.* According to the theory suggested by Adrian, Bronk, and Phillips (3), and maintained for a time by Hallpike and Rawdon-Smith (71, 78), the cochlear potentials are generated by nervous elements within the cochlea. The latter adopted this view as a result of experiments on section of the eighth nerve in cats (78). In 3 animals, 6 months after a unilateral operation, tests showed absence of the cochlear response on the side of the section. In one of them, all parts of the ear appeared histologically normal except the neural elements, of which only a few remnants remained. These investigators later returned to the membrane theory after experiments by Guttman and Barrera (68–70) and further work of their own in collaboration with Ashcroft (6) revealed cases in which nerve section failed to impair the cochlear responses, though it caused degeneration of the neural elements. These later observations were verified by Hughson, Thompson, and Witting (94).

3. *The Hair-Cell Theory.* According to this theory, the cochlear potentials arise in the hair cells of the organ of Corti. The possibility of a sensory origin of these potentials was mentioned in some of the early discussions by Wever and Bray (145, 147), but the first specific indication of such an origin appeared in the work of Howe and Guild (83, 84) on albino cats. Electrical tests of these animals revealed the absence of cochlear potentials, and a histological examination showed practically complete absence of the organ of Corti and its hair cells, with collapse of the cochlear duct and shrinkage of the stria vascularis, but no other cochlear abnormalities. Similar observations were later made by Davis and his collaborators (36, 110, 114) on both albino cats and waltzing guinea pigs, and by Hughson, Thompson, and Witting (93) on congenitally deaf Dalmatian dogs.

The hair-cell theory has been most explicitly and energetically supported by Davis and his associates (28, 36, 135). They reported in their studies of stimulation deafness and of cochlear destruction a close correlation between losses of cochlear response and losses of auditory acuity as shown by conditioned response tests (33). Fowler and Forbes (56, 57) likewise accepted the hair-cell theory on the basis of their studies of the effects of chemical substances upon the inner ear.

In recent discussions, Hallpike and Rawdon-Smith (79) have reviewed the evidence on the above 3 theories and have concluded in favor of the membrane theory, while Stevens and Davis (135)

have made an able defense of the hair-cell theory. The nerve theory as a complete explanation seems now to have no adherents, but it is generally agreed that a small amount of nerve potentials may be recorded from the cochlea as a ' contamination ' of the cochlear potentials proper. This fact was first pointed out by Davis, Derbyshire, and Lurie (34) and has since been confirmed (45, 165).

The hair-cell theory rests primarily upon the evidence from albino animals. Hallpike and Rawdon-Smith argued that this evidence is inconclusive, since in addition to hair-cell atrophy there was often collapse of the cochlear duct and a displacement of Reissner's membrane and the tectorial membrane from the usual positions. They did not accept the evidence of Davis and others (33) on stimulation deafness, for they maintained that the widespread damage which is typical of such experiments does not permit any reliable correlations of loss of response and hair-cell atrophy.

As more direct evidence for the membrane theory, Hallpike and Rawdon-Smith cited the experiment of Gatty and Rawdon-Smith (61) with a model consisting of a membrane of frog's skin bounded by fluids, and observations on the effect of eighth nerve section (6, 79). After nerve section, they observed cases in which cochlear responses were absent though the organ of Corti with its hair cells was morphologically normal, and other cases in which cochlear responses were present but the organ of Corti and its hair cells were grossly degenerate.

A further argument for the membrane theory, as against the hair-cell theory, is based upon recent experiments by Hallpike, Hartridge, and Rawdon-Smith (75). They studied the effects of a sudden change of the phase of a stimulating tone upon responses from the cochlea and brain stem. The cochlear responses were found to follow the phase change with considerable exactness. The nerve responses, on the other hand, usually showed a marked reduction in amplitude for a short interval after the phase change. The argument is that the ' silent interval ' in the nerve response reveals the presence of some resonant structure, probably the basilar membrane, in the path between stimulus and nerve action, whereas the absence of any such ' silent interval ' in the cochlear response places this response outside the resonant structures, and thus most likely in the movement of some membrane.

A different form of membrane theory is held by Eyster, Bast, and Krasno (50), in which it is suggested that the electrical effects arise as ' streaming potentials ' in the flow of liquid through a porous

membrane (cf. Leiri, 109). From various experiments, including the application of chemical substances to the round window, the passage of direct current through the cochlea, and exploration of pathological conditions of the inner ear, in all of which a histological examination followed the study of cochlear potentials, they concluded that "the development of cochlear potentials . . . does not depend on the structural integrity of the organs of Corti or associated structures."

It is far from easy to evaluate the evidence and arguments cited above, and at present a decision is largely a matter of personal judgment. There is no doubt that movements of fluids and membranes can generate electrical potentials, but in the writer's opinion such potentials in the cochlea are of negligible magnitude. The argument from the experiments on phase reversal includes 2 assumptions that may be questioned. The first assumption is that the basilar membrane is a resonant structure with damping so low that the membrane could not follow a change of phase. There is abundant evidence that damping in the ear is of high degree. The second assumption is that the 'silent period' in the brain-stem response is due to resonance in the basilar membrane or elsewhere in the end-organ. Other explanations are possible. The effects might be accounted for as a momentary inhibition of the usual impulses by other impulses generated by transients. That transients were present, and probably arose within the ear, is suggested by the form of the cochlear potentials as portrayed in sample curves, for this form appears to differ significantly from that of the stimulus as recorded with a microphone. The observations of Eyster, Bast, and Krasno are not in accord with those of most other investigators in this field. In the writer's opinion, the weight of the evidence is in favor of the view that the cochlear potentials are generated by the hair cells.

VI. AUDITORY NERVE RESPONSES

The responses of the auditory nerve may be observed at the nerve trunk or, under some conditions, at the round window. Both types of observation are attended with technical difficulties due to the presence of cochlear potentials. At the nerve trunk the cochlear potentials can be minimized by the use of a small electrode placed as far from the petrous bone as feasible and by the use of moderate stimulation and amplification. A shielded electrode of the Adrian-Bronk type is suitable, though the efficacy of the shield as such in excluding extraneous potentials is open to question.

When both auditory and cochlear potentials are present in the response, as often happens in spite of precautions, they nevertheless may be distinguished by particular features. A number of these, such as susceptibility to drugs, anesthesia, cold, death, and polarization, have already been mentioned. Derbyshire and Davis (45) regarded 2 tests as particularly useful: (1) the longer latency of the nerve response, and (2) the relation of its polarity to the stimulating wave. According to their measurements, the latency of the cochlear response is very small, 0.1 millisecond (ms.) or less, whereas that of the nerve response is 0.5 ms. or greater. They have further reported that the polarity of the cochlear response changes with that of the sound waves, while the polarity of the nerve response remains constant.

Davis and others (35) described an 'on-effect' for the nerve response, a large, momentary burst of impulses at the beginning of stimulation. This effect was said to be especially prominent at higher levels of the nervous system (28). A somewhat different 'on-effect,' as well as an 'off-effect,' was found in the cochlear response, and was supposed to reflect the resonance characteristics of the peripheral system. However, Snellen (133) was unable to observe such effects when precautions were taken to exclude clicks at the time of turning the stimulus on or off.

The nerve response is subject to equilibration; an initial large response falls off, at first rapidly, and then more slowly, until equilibrium is reached after about 7 minutes of steady maximal stimulation. The amount of decline of the response is said to vary systematically with frequency, and to be greatest around 1000, 2000, and 3000~. This observation, together with that of variations of initial amplitude for different frequencies, led Derbyshire and Davis to the conclusion that as the stimulating frequency is raised the nerve fibers go into 'alternation' at critical points. From observations of critical points in the frequency scale, Derbyshire and Davis (45) inferred that the 'functional refractory phase' of the auditory nerve is about 0.1 ms. This 'alternation' of activity is the same assumption made earlier by Wever and Bray (146) under the volley hypothesis in explanation of the high rates of impulses which they observed in the nerve. This hypothesis states that when the stimulating frequency becomes so great that the refractory period exceeds the period of the sound waves, a nerve fiber responds at every second wave, or every third, and so on. If different nerve fibers come into action for different waves, the total response can continue

to represent the stimulus frequency. However, the appearance of 'critical points' is unexpected, for it is difficult to see how the fibers of the nerve can have refractory properties so similar, and be excited so uniformly, as to give this result. The observation of critical points thus needs confirmation.

The nervous discharge is synchronized with the sound stimulus for all low and intermediate frequencies. The lower limit extends at least to 15~, and perhaps would extend farther were it not that below this point sensitivity falls off with great rapidity, and at the high stimulus intensities necessary the distortion is so serious that harmonic frequencies dominate the responses (Wever, Bray, and Willey, **165**). The upper limit of synchronism evidently depends in part upon conditions of observation, for various figures have been given. According to Derbyshire and Davis (**45**) this limit is between 3000 and 4000~.

Asynchronous responses continue to higher limits. In the opossum McCrady, Wever, and Bray (**115**) obtained responses from the eighth nerve up to 11,000~ in one animal and 15,000~ in another.

Early observations by Wever and Bray (**141**) and by Saul and Davis (**130**) indicated for the nervous response a different relation to intensity from that for the cochlear response. The nervous response was described as rising relatively slowly as stimulus intensity was increased. Wever and Bray suggested that the Weber-Fechner relationship might be found between cochlear and nervous responses. More recently, however, Derbyshire and Davis (**45**) described the function of the nerve response as similar to that of the cochlear response.

This problem of the functional relation of the nerve response to the cochlear response is complicated by irregularities in the form of the nerve potentials. Observation of these potentials at the round window shows that for very low tones an increase of intensity gives first an increase of amplitude and then a broader wave of response. Evidently at high levels the nerve impulses contributing to the response become more spread out in time (**165**).

For the nerve response, little is known as yet of the variation of sensitivity with frequency of stimulation. Observations of 'thresholds' by Derbyshire and Davis (**45**) gave results similar in form to those for the 'thresholds' of the cochlear response, but it is likely that the resemblance is more a function of the conditions of observation than of the phenomena themselves (**161**). The results

vary with the location of the electrode in the nerve, an observation which has been interpreted to mean that particular fiber tracts transmit impulses for particular tones (45).

Derbyshire and Davis made an extensive study of the nerve responses to clicks. Such stimuli produce multiple potentials, usually 3 in number, which are distinguished by their latencies. The latency of the first of these potentials was said to bear a constant relation to the negative peak of the cochlear response. Hissing sounds and tones produce masking effects that vary for the different potentials according to their latencies and also according to the frequency of the masking sounds. These observations on masking were regarded as additional evidence that different fibers transmit impulses for different frequencies (45).

Nerve Excitation. Three hypotheses have been advanced to explain the process of nerve excitation. These respectively regard the process as mechanical, electrical, or chemical.

According to the mechanical hypothesis, it is assumed that changes of pressure are communicated to the nerve terminations at the hair cells and excite these terminations directly. It follows that the cochlear potential, arising in the deformation of the hair cell, is incidental to the excitatory process.

According to the electrical hypothesis, the cochlear potential itself excites the nerve terminations.

According to the chemical hypothesis, a deformation of the hair cells liberates a chemical substance which acts as a mediator in the excitation of nerve fibers.

Davis and others (36) for a time accepted the electrical hypothesis, but later, in consideration of the relatively long latencies of nerve responses, gave it up in preference to the chemical mediator theory (44, 45). It is not clear, however, how the chemical mediator theory can account for time relations of excitation so precise as to give synchronous nerve volleys as rapid as 3500 or more per second.

VII. Responses of the Central Nervous System

A number of experiments have studied electrical responses from the acoustic tracts of the central nervous system. These responses have been observed at the cochlear nucleus, traced through the trapezoid body and lateral lemniscus to the inferior colliculus, thence through the inferior quadrigeminal brachium to the medial geniculate body, and finally by way of the acoustic radiations to the temporal cortex.

The effects are found alike in homolateral and contralateral pathways (145, 147). Saul and Davis (129) reported that the responses were greater in the contralateral tracts, but at the lateral lemniscus Kemp and Robinson (102) found homolateral and contralateral effects to be equal. In the various acoustic tracts the responses are highly localized: usually only a slight movement of the electrode causes a great change in amplitude (40, 130, 147). At some points, perhaps due to the presence of cell-bodies, the potentials exceed those recorded from the nerve trunk (129).

The central effects are highly susceptible to such adverse conditions as drugs, anesthesia, fatigue, circulatory deficiency, and death. Their latencies are relatively long, and increase at higher levels of the system, due to the increase in length of pathway and the addition of synaptic junctions. The time of synaptic delay was calculated by Kemp, Coppée, and Robinson (98, 101) as in the region of 0.8 ms.

The relations to stimulus frequency vary with the level of the nervous system at which the observations are made. In general, the limits of frequency for which potentials may be obtained are below the limits shown by the nerve trunk, and decrease further as higher neural levels are reached. Kemp, Coppée, and Robinson (101) observed synchronous responses up to 2500~ at the trapezoid body, but only to 1000 or 1500~ at the inferior colliculus; for higher frequencies the responses were for a time partially synchronous, then wholly asynchronous, and finally absent.

At the trapezoid body the upper limit of frequencies for which asynchronous responses were found was 4000~, and at the inferior colliculus this limit was 1000~; higher tones gave only a momentary effect as the switch was opened (100). These results were obtained in the cat. McCrady, Wever, and Bray (115), working on the opossum, obtained responses from the trapezoid body up to 9000~. The difference is probably as much a function of recording apparatus as of animal species. Gerard, Marshall, and Saul (63) reported synchronous responses in the region of the corona radiata to a tone of 660~. At the cortex Davis (28, 29) reported no synchronism to tones, though a succession of clicks was reproduced up to 100 per second.

Evidence for the specific action of different fiber groups for different stimulus frequencies was brought forward by Kemp and Coppée (99) for the inferior colliculus and by Gerard, Marshall, and Saul (63) for the cortex. Kemp and Coppée stimulated with clicks, and found the usual responses to be obliterated by tones whose necessary frequency varied systematically with electrode position.

The relation of the central responses to stimulus intensity was studied at the lateral lemniscus by Kemp, Coppée, and Robinson (101). They found the magnitude of the potentials to increase with intensity, sometimes in a regular and sometimes in a rather irregular manner, and finally to pass through a maximum. The function differed with the position of the electrode, and a new position often yielded responses greater than the maximum obtained from a previous location. Though these writers do not point it out, their more regular functions show through the greater part of their course a logarithmic relation between stimulus and electrical responses; as will be discussed later, this relation is important for the Weber-Fechner law. These investigators found that for low tones the sensitivity at the lateral lemniscus is greater than for cochlear responses at the round window.

Further studies by Kemp and Robinson (102) at the lateral lemniscus revealed a number of characteristics of binaural stimulation. For low intensities, stimulation of both ears equally gave amplitudes of response which represented a simple addition of effects. There was no appreciable difference in the latencies for homolateral and contralateral pathways. No binaural masking was observed; and if the 2 ears were stimulated at slightly different times there was no inhibitory effect, and the proper latencies were preserved. These results are of particular value for the problem of the binaural localization of sounds.

Much of the evidence on central nervous processes is fragmentary, and therefore difficult to evaluate. There is little question that the failure of agreement of many of the observations is partly due to differences in technique and in sensitivity of recording apparatus. Differences in sensitivity will affect particularly the limits of observable responses.

VIII. Embryological Development of the Responses

The electrical responses of the ear have been employed in the study of the development of auditory function in relation to the structural differentiation of the ear. This investigation has been carried out on the opossum, one of the few mammals in which the ear is immature at birth. McCrady, Wever, and Bray (115) observed the cochlear responses in various members of a single litter of pouch-young at intervals of 59 to 82 days after birth. The results showed responses in the youngest animals to tones of middle frequencies only (about 500 to 5000~), and in older animals a progressive

extension of the range to include both lower and higher tones. At the same time, sensitivity increased greatly for all tones and especially for high tones. Maximal sensitivity was first shown in the region of 2000~, but with growth this maximum shifted upward and finally lay in the region of 7000~, where it is still found in the adult opossum. At 80 to 90 days of age the ear appeared to have reached its maturity. These results are confirmed by observations on a second litter (unpublished).

Studies of reflex movements in response to sounds in the above animals yielded results in general agreement with the electrical studies. Moreover, the growth of function correlates well with the structural differentiation of the organ of Corti, which begins in the distal position of the basal coil of the cochlea and thereafter extends in both directions.

IX. PHYLOGENETIC STUDIES

A number of species of animals have been investigated by the electrical method, with results that are pertinent to a consideration of hearing in relation to the phylogenetic series. The species studied include several insects, and one or more representatives of each of the classes of vertebrates.

Insects. Only a few species of insects possess an organ of a form and complexity such as to indicate a true auditory function. The most familiar of these, the grasshoppers, katydids, and crickets, are found in a single order, the Orthoptera. On the forelegs of katydids and crickets and on the first abdominal segment of grasshoppers is borne a special organ for sound reception, which consists of a tympanum in relation to specialized sensory cells (scolophores) and a sensory nerve. Electrical responses may be recorded from either the nerve or its central ganglion.

Crickets of the species *Gryllus assimilis* as studied by Wever and Bray (151) usually gave responses over the range of 300 to 8000~. Katydids of the species *Amblycorypha oblongifolia* showed a greater range for high frequencies; one specimen responded from 800 to 45,000~. Grasshoppers (*Arphia sulphurea*) responded from about 300~ to about 20,000~ (142). Measurements of absolute sensitivity in the grasshopper showed this ear to be very deficient in response to low tones, but to be as sensitive to high tones as the ear of man (122, 142).

The tympanal organs so far studied give only asynchronous potentials over the range of frequencies to which they respond. This

may be true because the number of nerve fibers supplying these organs is small. At any rate, certain other sensory endings of insects, of a much simpler form but more richly supplied with nerve fibers, give synchronous potentials. The cercal apparatus of the cockroach (*Periplaneta americana*) and also of the cricket (*Gryllus domesticus*), which consists of sensory hairs and other endings supplied by the cercal nerve, was found by Pumphrey and Rawdon-Smith (**123, 124**) to respond readily to sound waves of low and intermediate frequency, up to about 3000–4000∼. At the lower end of this range the responses were often synchronized with the stimulus, but at times other relations, as frequency-doubling, frequency-halving, etc., were observed. These results are significant for their resemblance to the frequency relations of the electrical responses in animals with a much more elaborate acoustic apparatus.

Fishes. As there is good evidence for hearing in certain fishes, we should expect to obtain electrical responses from their ears. So far, however, the only positive results are the early observations of Piper (**118–120**), mentioned above. Wever and Bray have made attempts to record potentials in the goldfish, but with results which, so far, are uncertain.

Amphibians. In this class of vertebrates only the frog has been studied. Ashcroft and Hallpike (**4, 5**) obtained responses from the saccular nerve when the platform bearing the preparation was jolted or caused to vibrate by applying the stem of a tuning fork. Synchronous responses were secured up to 512∼, and momentary responses of uncertain synchronism at 1024∼. Since air-conducted sounds were ineffective, Ashcroft and Hallpike concluded in favor of the view that the saccular organ of the frog and also of higher forms serves a distinct vibratory function: the reception of bone-conducted sounds. However, Wever and Bray (**154**), in experiments on the bullfrog (*Rana catesbiana*), observed responses which appeared readily to air-conducted sounds. The range of frequencies for which responses were obtained extended to 500 and sometimes 600∼, a limit which agrees well with that observed by Ashcroft and Hallpike. In view of these results, the conclusion regarding a special vibratory function for the saccule must be regarded with some doubt.

Reptiles. As already referred to, Foà and Peroni (**53**) recorded from the facial-auditory nerve of the giant sea turtle (*Thalassochelys caretta*) and obtained responses around 50–60 per second, irrespective of the stimulus frequency. From the separate cochlear nerve of the common painted terrapin (*Chrysemys picta*) Wever and Bray (**148**) observed large synchronous responses up to 500∼, and

responses of diminished magnitude up to 1000 and sometimes 1200∽. The sensitivity to low tones seemed somewhat greater than that found for mammals, but no precise measurements were made. The ear of the turtle appears to be superior to that of the frog in range, and possibly also in sensitivity.

Birds. Among birds, only the pigeon has been studied. Wever and Bray (153) recorded responses from the round window over a range of frequencies which usually extended from 100 to 10,000∽, but was sometimes slightly wider. These limits are in close agreement with the limits of hearing obtained in conditioning experiments. The responses were synchronized with the stimulus frequency and reproduced its wave form. The relation to stimulus intensity is fairly regular, and resembles the functions obtained for mammals except for a characteristically lower slope. The slope is usually around 0.4, as compared with slopes near unity for mammals; this indicates a relatively low efficiency in sound transmission.

The sensitivity of the pigeon's ear as revealed by electrical responses is only moderately inferior to that of cats and guinea pigs at low intensities of stimulation, but at high intensities it becomes decidedly inferior. Sensitivity is greatest near 2000–3000∽, and falls off particularly rapidly for higher tones.

Mammals. Studies of electrical responses among mammals include 8 species: opossum, rat, rabbit, guinea pig, cat, dog, monkey, and man. Of these, only the opossum, guinea pig, and cat have been extensively dealt with.

The opossum is of particular interest because it is one of the most primitive mammals. Cochlear responses are found for a considerable range from about 100 to 25,000∽, and show a maximum of sensitivity around 7000∽. Nerve responses have been observed up to 11,000 and 15,000∽. These limits agree well with results for pinna reflexes and startle reflexes in response to tones; the maximum range over which such reflexes have been observed is 100 to 19,000∽ (115).

A few observations have been made of nerve responses in rats and rabbits (140), but the quantitative data are very limited.

Cochlear responses have been observed in the guinea pig to tones from 5 to 25,000∽ (158). At all but the lowest frequencies, the responses reproduce with reasonable faithfulness the wave form of the stimulus. Between 5 and 15∽ the distortion is great, and the observed responses are made up of harmonics of the stimulating frequency (165). For tones of the middle range the sensitivity is relatively high, and reaches its maximum in the region of 600∽ (155).

The cat has been used for electrical studies more extensively than

any other animal. The limits of cochlear responses extend from 35∼ or lower up to 30,000∼. The sensitivity compares favorably with that of the guinea pig; in general the sensitivity of the cat is poorer than that of the guinea pig for low frequencies but better for high frequencies (158, 161).

Cochlear and nervous responses have been observed in both dogs (8, 11, 19, 25) and monkeys (62, 63), but no extensive quantitative data have been obtained.

Several attempts have been made to obtain cochlear responses in man. An early effort by Wever and Bray to record such responses from the exterior of the ear gave no results. Grossmann and Spielmann (65), Van Gilse (64), and others likewise have reported negative results. In one experiment, Fromm, Nylén, and Zotterman (58) used patients with perforations in the drum that were large enough to give access to the round window, but the same methods that yielded responses in the cat were unavailing. In a later series (59), with a different arrangement of amplifying apparatus, positive results were obtained from 2 patients—in one of these from both ears. In the best of these experiments observations were made over a range of 200 to 8192∼. The responses were much weaker than responses obtained similarly from cats, but the difference was probably due at least in part to the fact that the patients were defective in hearing.

If a technique for human recording could be developed with a practical degree of sensitivity it would undoubtedly be of service in clinical diagnosis. It is disappointing that attempts so far have been largely barren of results. The failures seem unaccountable except on the basis of variations of technique or defective hearing in the subjects, for we know of no important structural differences between human ears and the ears of cats, guinea pigs, and monkeys, and thus have every reason to expect large cochlear potentials.

The results on various species of animals are hardly sufficient as yet to form a complete picture of the phylogenetic development of auditory function. The results on the insects, in conjunction with those from behavioral studies, are of interest in revealing the characteristics of a peculiarly isolated form of vibratory receptor. The evidence on the vertebrates, so far as it goes, indicates that within this subphylum the development of hearing runs a fairly direct course. From fish to mammal, it is marked by a reasonably consistent gain in frequency range. Whether there is also a gain in maximal sensitivity remains to be learned, but there is little doubt

that as development has proceeded the region of greatest sensitivity has shifted in the direction of the higher frequencies.

In the various animals, the results from electrical studies are consistent with what we know of hearing on the basis of behavioral tests. Comparisons must be made with caution, since the results of the electrical studies are always limited to some extent by recording apparatus, and also because the animal's reactions to sound are determined by central and motor as well as by peripheral processes (140). However, the conditions permit a comparative judgment of the limitations which the peripheral mechanism imposes. In the animals adequately studied by both methods (cat, opossum, and pigeon), the limits of electrical responses bear to one another a relation similar to that shown in behavioral studies.

X. STUDIES OF THE MIDDLE EAR

Crowe and Hughson (19, 88) first pointed out the usefulness of the electrical method for a study of the function of particular parts of the ear, and the method has been applied by them and others in investigations of the drum, ossicles, tympanic muscles, middle-ear pressure, and infectious conditions of the middle ear.

1. *Ear Drum.* Early studies by Crowe and Hughson (19, 88, 90) revealed no appreciable change in responses after simple incision of the drum or the damping of limited areas with vaseline or pledgets of cotton. Covering the whole surface, however, impaired the responses, especially those for low tones. Recently Bordley and Hardy (10), with improved methods of observation, found that small but consistent impairments result from drum incisions. The losses vary with the site of the incision and its extent and are relatively greater for tones of low frequency and of high intensity. Their results agree closely with audiometric studies of the effects of simple traumatic rupture of the drum in man.

2. *Ossicles.* An interruption of the ossicular chain has a profound effect upon the electrical responses to air-conducted sounds (Crowe and Hughson, 19), but affects the reception of bone-conducted sounds relatively little (Guild, 67; Wever and Bray, 152). The losses for air conduction are about 40–60 db., and are somewhat greater for high than for low tones. Any mechanical interference with the movements of the ossicles impairs the responses (19).

3. *Tympanic Muscles.* In cats and guinea pigs under light anesthesia, the muscles of the middle ear frequently show momentary contractions (Davis and others, 33). Also, the contractions are

elicited by stimulating the pinna, the external meatus, and the facial nerve (Hughson and Crowe, **90**; Hughson and Witting, **95**). Hallpike (**72**) reported reflex contractions to sounds, but Hughson and his collaborators (**90, 95**) were unable to confirm him. The muscle contractions, whether 'spontaneous' or elicited artificially, cause marked reductions in the magnitude of cochlear potentials. The amount of reduction may be made to vary according to the intensity of stimulation of the pinna (**162**).

The function of the tensor tympani muscle was investigated in the cat by Crowe, Hughson, and Witting (**19, 21, 90**) by artificially reproducing its action. Tension was exerted on the tendon by means of an attached thread and observations were made of the resulting reductions of response. Further results by a slightly different method were obtained by Wever and Bray (**160**). Under most conditions of stimulation, the amount of diminution of the responses bears a simple relation to the degree of tension; as tension is increased the responses fall, at first rapidly, and then progressively more slowly. If high intensities of stimulation are employed, so that the region of overloading is reached, the tension curves take various forms depending upon the degree of overloading. Of particular interest is the case in which the overloading is severe and the position on the intensity function is beyond the maximum; for such a stimulus the application of tension first causes an increase in the cochlear response, and finally, if sufficient tension is applied, is followed by the usual decrease. These observations suggest that overloading appears beyond the middle ear, possibly in the responses of the hair cells, and that tension, by reducing transmission, reduces the severity of overloading.

The effects of tension upon the responses varies with frequency of stimulation in a manner that suggests a relation to sensitivity (**160**).

Wiggers (**166**) studied the effects of 'spontaneous' contractions of the muscles upon the cochlear responses. As a result of contractions he observed considerable reductions of response for tones below 1000~, and the reductions were greater the lower the frequency. On the other hand, the transmission of tones between 1300 and 1800~ was enhanced, while transmission above 2000~ was unaffected. As this work was done on the guinea pig, there is no necessary disagreement with the results already mentioned for the cat. It is possible that the variation with frequency is related to the sensitivity of the animals, though the results are not sufficient to determine this point.

No immediate effect upon the cochlear responses has been observed as a result of denervation of the tensor tympani muscle, or cutting its tendon (19, 163).

Relatively little information is available on the action of the stapedius muscle. Hughson and Witting (95) reported that tension upon its tendon produces more marked effects than the corresponding treatment of the tensor tympani muscle. They reported further that overloading develops earlier when the tendons of both muscles are divided.

Stevens, Davis, and Lurie (136) reported, in a single case, a great and evidently permanent loss of responses as a result of sound stimulation in an animal whose tensor tympani tendon had been divided. This observation has not been verified.

Culler and Finch (24), in a stimulation deafness experiment, obtained impairments which were especially pronounced when the tensor tympani tendon was cut. On the other hand, Hughson and Witting (95) observed the effects of detonations to be less when the tensor and stapedius tendons were divided than under normal conditions. There is no real discrepancy between these observations if we admit a twofold function for the tympanic muscles, one of protecting against excessive stimuli, the other of adding strength and rigidity to the ossicular system. In the presence of detonations the muscles give no protection because their latencies are so long that damage is done before they begin to act. Hence their absence is no handicap; rather, it is a help, for when the ossicles are in a more fragile condition they fail first and the sounds do not reach the inner ear to do more serious damage there. In the stimulation deafness experiments, in which the stimuli are continuous, the muscles are able to exert their protective action, and hence their absence is a disadvantage.

4. *Middle-Ear Pressure*. The relation of pressure in the middle-ear cavity to the electrical responses was studied by Thompson, Howe, and Hughson (138). They varied the pressure by means of an air pump connected to a needle fixed in the Eustachian tube, and recorded the resulting pressure with a manometer connected with a second needle cemented in the auditory bulla. The results showed impairments for both positive and negative pressures in amounts which increased with the departure from normal. The effects were greater for high and low tones than for tones of the middle range.

5. *Pathology*. Pathological conditions of the middle ear are occasionally encountered in animals, especially in cats and rats.

Such conditions usually cause a marked reduction in cochlear responses (**91, 114**). Reductions as great as 40 to 60 db. have been reported as a result of middle-ear infections, particularly those in which the ossicles were seriously affected. Responses to low tones are impaired somewhat more than those to high tones, and over-loading for strong stimuli usually fails to appear (**95**).

XI. STUDIES OF THE INNER EAR

1. *Round Window.* A number of efforts have been made to determine the function of the round window. Hughson and Crowe (**19, 89**) exerted pressure on the round-window membrane with a pledget of cotton, and observed an improvement of responses. They then tried to obtain permanent effects by grafts of fascia, periosteum, and bone in the round-window niche, and reported significant improvements of response (**89, 90**). These experiments were made on cats, but the suggestion was advanced that such grafts in human ears might serve for the relief of some kinds of deafness.

Finch, Culler, and Girden (**25, 52**) repeated the above experiments, though with a somewhat different technique. A soft-gum plug was placed in the round-window niche of dogs, and tests were made by both electrical and conditioning methods. Both methods agreed in showing impairment as a result of the plug (**26**).

Hughson and Crowe had already pointed out that simple blocking of the round window with cement produced no change, and it is likely that the gum-plug experiment is more comparable to this procedure than that designed to produce positive pressure. Thus the degree of disparity of the experiments of Hughson and Crowe and those of Finch, Culler, and Girden is not yet clear. Recently, Hughson (**87**) has proceeded with graft experiments in human patients, with results which cannot yet be suitably assessed.

More drastic procedures, such as puncturing and tearing the round-window membrane, have yielded various results (**19, 88, 167**). Davis and others (**36**) reported impairments which in different trials ranged all the way from nearly none to 90%. It is likely that in these experiments there were large variations in the operative damage. Hughson and Crowe (**90**) pointed out that at times the manipulations caused fracture of the basal osseous lamina, and the serious losses no doubt are to be attributed to this cause.

2. *Intralabyrinthine Pressure.* Observations on the effects of pressure on the round-window membrane raised the problem of the relation of intralabyrinthine pressure to the action of the ear, and

cast doubt upon the general belief that an increase in such pressure interferes with hearing. Investigations by Hughson and Crowe (**85, 90**) showed that electrical responses are impaired if intralabyrinthine pressure is sharply decreased, but are unchanged if pressure is increased. The method was the intravenous injection of hypertonic salt solution for reducing the pressure, and distilled water or hypotonic salt solution for raising the pressure. A cochlear manometer was used for direct recording of intralabyrinthine pressure. This pressure seems to be independent of the air pressure of the middle ear.

Culler, Finch, and Girden (**26**) injected saline solution intradurally in dogs and cats, and observed a decline of cochlear responses which varied in different trials from 2 to 27 db. No measurements were made of the intralabyrinthine pressure, but a marked bulging of the round-window membrane showed that this pressure was raised. The same procedure produced corresponding losses of hearing as tested by conditioned responses.

3. *Effects of Drugs.* Many chemical substances, such as cocaine, salicylic acid, chloroform, and strong saline solutions, when applied to the cochlea, have been found to impair its responses (**2, 68**). Injection of these substances through the round-window membrane produces the most immediate effects, because the substances reach the essential tissues more promptly, and doubtless also because in many instances serious mechanical damage is done at the same time. Direct mechanical injury is avoided if the substances are instilled into the bulla, or if minute quantities in the form of drops or crystals are placed on the round-window membrane (**77**). The reductions shown by the cochlear response have been confirmed by conditioning tests (**55, 57**).

Fowler and Forbes (**56**), after application of various substances to the round window, noted a progressive decrease of responses in order of frequency. High tones were impaired earlier and to a greater degree than low tones, and histological examination revealed a marked destruction of hair cells and sometimes of other tissues. The histological changes were usually most marked at the basal end of the cochlea, and the results were interpreted as supporting a place theory of cochlear action.

Wever and Bray (**159**) found the series of changes after application of sodium chloride to be somewhat more complicated. Frequently there is an initial augmentation of responses, which perhaps can be attributed to an alteration of electrical conductivity. The responses to all frequencies then suffer a rapid decline. However, in

most cases the responses to high tones continue their decline for a longer time, and hence, after an interval, a systematic relation to frequency is established. The application of sodium chloride to a minute hole in the apex of the cochlea likewise causes general impairment, but of smaller degree.

XII. The Localization of Responses in the Cochlea

The experiments just described, in which chemical substances are applied to the inner ear, have a particular interest for the problem of cochlear localization. Fowler and Forbes ascribed their results to a progressive involvement of sensory endings as the material diffused from the base of the cochlea toward the apex, a view that was supported by histological evidence of degenerations in the basal regions. They interpreted the results as evidence for localization in the cochlea, with high tones in the base and low tones in the apex. However, the results obtained by Wever and Bray complicate the problem somewhat, and if the place theory is retained some additional process must be postulated to account for the early losses suffered by all tones.

Other evidence on the problem of localization in the cochlea is gained in experiments on cochlear destruction, stimulation deafness, and the selective placement of electrodes in connection with the study of cochlear responses.

1. *Cochlear Destruction.* General and profound injuries to the cochlea invariably result in a cessation of auditory electric responses (144, 145). Partial destructions sometimes produce great losses, but at other times cause changes that are surprisingly small. The differences are no doubt due to variations in the locus and amount of damage to essential structures. The smaller losses indicate that a part of the inner ear can continue to function after grave mechanical disturbances, including complete opening of perilymphatic and endolymphatic ducts. Bast and Eyster (7) reported that the apical, third, and second turns of the cochlea of the guinea pig could be removed without abolishing the responses. Potentials to low frequencies were markedly reduced, but those to high frequencies were only slightly affected.

Several attempts have been made to produce highly localized cochlear lesions. The results have varied, probably for technical reasons. Hughson, Crowe, and Howe (91) and later Hughson, Thompson, and Witting (92) reported experiments in which a drill hole was made part way through the cochlear wall, and the base of

the hole scorched with a high-frequency cautery. They found most of the lesions to have the greatest effect upon high tones. Lesions in the apical region had relatively little effect. In chronic experiments in which several days elapsed between operation and testing, the impairment was more profound than in the acute experiments. Histological study of the chronic cases showed relatively diffuse atrophy of the organ of Corti and nerve fibers, possibly due to the spread of toxic products of the burned tissues.

Several experimenters have made simple mechanical lesions, and have observed an impairment of electrical responses (Van Gilse, **64**; Hinnen, **82**). Dubner, Gerard, and Kobrak (**46**) and later Kobrak, Lindsay, Perlman, and Dubner (**103**) observed the effects of cochlear lesions upon middle-ear reflexes, nerve potentials, and cochlear potentials, and found them to have the greatest effect upon the middle-ear reflexes and the least upon the cochlear responses. After a simple lesion, Bast and Eyster (**7**) found usually a loss of transmission of one-third to one-half, and after a time a rise to nearly normal. Culler (**23**) produced a lesion by drilling at the place that other explorations had indicated as the 'focus' for 200∿. The results of both electrical and conditioning tests showed great loss of acuity for all test tones, and somewhat greater losses in the region of 200–250∿.

Stevens, Davis, and Lurie (**136**) produced lesions by drilling into the cochlea in guinea pigs, and obtained impairments of cochlear responses which they regarded as correlated with the regions of destruction of sensory elements as later revealed by histological examination. The results were not given in detail, but were summarized in a figure which expressed the relation between cochlear damage and impairment of response. However, the sample data that were given show great irregularities, and the reviewer finds himself little in agreement with the authors' plottings of them.

It is evident that this type of experiment is far from satisfactory. It is difficult to produce a simple lesion because in a structure as fragile as the cochlea such manipulations as drilling and cauterizing, even though done with care, may cause injuries beyond the immediate site. Even if histological examination is made afterwards, it is difficult to evaluate structural damage and to differentiate functionally normal and abnormal cells. On the other hand, it is often surprising that a considerable amount of damage can be done without abolishing the cochlear responses. In this relation it should be borne in mind that the peripheral part of the ear is approximately a linear system,

and a loss of half the elements active for a given stimulus should at most produce a reduction of about 6 db. The actual loss observed for a given amount of destruction will, of course, vary with the effectiveness with which the electrode taps the responses of the various elements.

2. *Stimulation Deafness.* Since the early work of Von Stein and Wittmaack, many experimenters have exposed animals to loud tones over varying periods of time in order to study the effects upon histological structures of the ear, and, in a few instances, upon the functional condition of the ear so far as it could be ascertained by the crude tests then available. Recently this type of experiment has been revived with cochlear response and conditioning tests to determine the extent of damage (24, 33, 163). The results of various investigators are in fair agreement. Tones of low frequency have little or no effect, while tones of 2500~ or over sometimes produce impairments whose locations in the frequency scale bear little if any relation to the stimulating tones. In practically all cases there is a reduction of responses for tones over a wide frequency range (164), and a marked involvement of intermediate tones, presumably tones in the region of greatest sensitivity for the animal (33).

Results somewhat similar to those with tonal stimuli have been obtained with detonations (92). It has been reported that the cochlear responses for high tones are affected somewhat more than those for low tones, but these losses show no close relation to the site of damage as revealed in histological sections. There is evidence that, for pure tones and detonations alike, the most severe histological changes occur in the middle portion of the cochlea.

The relation of stimulation deafness to the effects already described under fatigue has not been made clear. It seems likely, however, that the 2 effects are successive stages of the same process of tissue damage.

3. *Effect of Electrode Position.* Hallpike and Rawdon-Smith (76, 77) first reported a systematic variation of cochlear potentials as a function of the position of the active electrode on the bony wall of the cochlea. They found that as the electrode was moved from base to apex the responses to all tones progressively decreased. This change was attributed to the insulating properties of the bony capsule. However, by the use of a technique devised to neutralize the effects of insulation, they were able to demonstrate a differentiation of the responses with respect to frequency. At base and apex small holes were drilled nearly through the bone, and the recording

electrode made contact with a drop of mercury in the hole. They reported that responses to low tones were 3 or more times as strong when recorded from the apical electrode as from the basal, and, contrariwise, that high tones were about 4 times as strong when recorded from the basal electrode.

This observation has been confirmed by several investigators. Culler (22), indeed, has extended the observations by recording from a great many points on the cochlea of the guinea pig, and has presented a map which shows the focal regions of various tones from 60 to 7000∼. Unfortunately, only a few of the results upon which the map was based have been published, and these show considerable irregularities. Until a full report is made, or others repeat the work, it will not be possible to evaluate these observations.

4. *Nerve Sectioning.* An experiment of considerable theoretical interest is the selective sectioning of fibers of the auditory nerve. Three attempts at such an experiment have been made, but the results to date are not extensive. Wever and Bray (156) carried out partial sectioning of the eighth nerve in cats, and made observations of the effects upon responses from the contralateral cochlear nucleus (with the contralateral cochlea completely destroyed). The experiment is complicated by the presence of nonauditory fibers and the danger of cutting the cochlear artery. The results varied as expected; some sections caused no change, some produced a loss of the highest tones, and others showed a general and progressive loss of all tones. These results may be interpreted as indicating either a relatively specific representation of high tones in the nerve, or a requirement by such tones of a relatively large number of fibers. So far as they go the observations agree with those of Dandy on the effects of partial section of the eighth nerve in man for the relief of Ménière's disease.

Fromm, Nylén, and Zotterman (59) and Hughson, Thompson, and Witting (94) recorded from the cochlea, and observed no diminution of responses after severing successive strands of the nerve.

XIII. Auditory Theory

The original incentive for the investigation of the electrical responses of the acoustic system was provided by questions of auditory theory. These questions, though not fully answered, are greatly clarified by results now at hand.

There is little doubt that the cochlear response represents the form of the stimulus as it is received and modified by the peripheral mechanism of the ear. Its study reveals limits of frequency, fidelity,

and intensity for the transmission process—limits which account in part for limitations of auditory perception and response.

The transformation of intensity from a linear to a logarithmic scale, as subsumed in the Weber-Fechner law, does not appear in the cochlear response, and must be sought in further processes. As was brought out in Section VI, it is disputed whether this relationship enters between cochlear and nervous responses. At any rate, the evidence of Kemp, Coppée, and Robinson (**101**) indicates that a transformation of the required sort has already taken place by the time the lateral lemniscus is reached, though these writers themselves have not discussed this application of their observations. It was pointed out above that in their more regular functions the amplitude of response is proportional to the logarithm of stimulus intensity through the greater part of the range. Moreover, the departures from this relation at the 2 extremes of the function are in the same directions as the known departures from the Weber-Fechner law in hearing.

Two further problems lie in the foreground of attention. These are the spatial distribution of action in the cochlea and the representation of frequency and intensity in the auditory nerve.

Several results of the electrical studies bear on these problems. In the preceding section the evidence on the first was detailed. This evidence indicates a limited degree of differentiation of response in the cochlea with respect to stimulus frequency. High tones seem fairly well localized in the basal region of the cochlea, and low tones much less specifically localized in the remaining regions.

There is also evidence on the problem of the spatial representation of intensity. It is probable that all tones, and especially those of low frequency, are spread somewhat over the basilar membrane even when of moderate intensity. From the extensive impairments obtained in stimulation deafness experiments it seems certain that the spread of action increases with intensity, and that tones of great strength involve nearly the whole of the cochlea.

In regard to the second problem, we have as the chief feature of the auditory nerve response its representation of the frequency of the stimulus. It is agreed that below some limit, possibly 1000~, the stimulus frequency is reproduced with at least fair fidelity by the individual nerve fiber. Above that limit the frequency is still represented, but only, it is presumed, by the combined action of many fibers. If refractory periods differ for the fibers set in action by a given tone, and if these fibers are unequally stimulated, their times of

firing will not be mutually synchronous, but each individual impulse will be in time with some wave of the stimulating sound. The total pattern established by numerous fibers will properly represent the stimulus frequency. This, the volley hypothesis, accounts for the high frequencies observed in the nerve without requiring refractory periods below those known for other nerve fibers (Wever and Bray, 146).

On this hypothesis, the highest frequencies so far observed in the nerve and in central tracts are not necessarily the highest represented. In the first place, the limits found are a function of the sensitivity of recording apparatus, and an improvement of sensitivity has usually revealed frequencies above those previously observed. A second condition is the type, size, and location of the electrode. It is obvious that an electrode so small as to record from only a few fibers will not present a true picture of the activity of a large bundle of fibers firing in rotation, and that this deficiency will be accentuated if fibers which represent essential parts of a pattern of frequency are spatially separated. The spatial separation of functionally related fibers will doubtless grow more serious as higher levels of the nervous system are reached. Finally, unless the transition from synchronism to asynchronism is particularly abrupt, it will be difficult to decide where the transition may be said to be, and also what degree of synchronism may yet have significance in the representation of characters of the stimulus.

The Place Theory. Opinions differ as to the theoretical bearing of the relations just indicated. Davis and his colleagues (34, 45), apart from a few remissions, have adhered to the place theory. They have correlated stimulus frequency with the locus of response in the cochlea and hence with specificity of nerve fiber action, and have correlated stimulus intensity with the spread of action in the cochlea and hence with the number of nerve fibers in operation. They have regarded the frequency of nerve responses as an incidental affair, an epiphenomenon, so far as pitch and intensity are concerned, though it is admitted a possible rôle in the binaural localization of sounds.

Now, as throughout its history, this type of theory has had wide acceptance. A common argument is that the theory has the advantage of simplicity. However, as Wever and Bray (146) have pointed out, its simplicity is apparent only. A closer consideration shows that frequency of nerve impulses cannot be incidental to pitch perception even in a place theory. This condition arises from the fact that tones spread over the basilar membrane, and thus overlap so

that the excitation of a given nerve fiber as such gives no clear clue to pitch. A definite clue can be provided only by some differential action of the various fibers involved in the response to a tone, to the end that not only the locus but also the form of the basilar membrane activity is represented. If the form is represented, then it will be possible through central processes to appreciate the pitch, in terms, perhaps, of the maximum point of stimulation as first suggested by Gray.

As is well known, the possible variations in nerve response are limited, and the only reasonable assumption for differential action of the fibers is through variation in the frequency of impulses. Pitch, then, on this supposedly simple theory, is a complex derivation of place and frequency. It is remarkable that these implications of the place theory have never been worked out by its adherents.

The Resonance-Volley Theory. Wever and Bray (**146, 156**) have developed the possibilities for auditory theory in a different direction, by a combination of fundamental features of the traditional place and frequency hypotheses. The evidence on the localization of action in the cochlea is accepted at its face value as indicating only limited representation in terms of place. Likewise, the evidence of the representation of tonal frequency in nerve activity is accepted as a significant feature of acoustic response. The volley hypothesis is used to explain the presence in the nerve response of frequencies much above those expected from a single fiber.

In this theory, pitch has a twofold representation. Low tones are represented by frequency, high tones by place, and other tones by a combination of the two. These suppositions follow the available evidence. Place alone serves for the high tones because localization in the cochlea for these is relatively specific, but the asynchronous character of the nerve volleys makes frequency representation indefinite. Frequency alone serves for the low tones, because they spread widely in the cochlea, yet in the nerve response they are represented with high accuracy. For the great body of intermediate tones both forms of representation are available.

Loudness is correlated with the total number of impulses in the nerve per unit of time, and thus relates both to the number of active fibers and the rates at which they enter successive volleys. These assumptions agree with evidence on intensity representation in cutaneous and other nerves, and embrace Adrian's intensity-frequency principle.

Other aspects of auditory experience that the resonance-volley theory explains include the number of cycles necessary for pitch

perception, the variations of pitch discrimination with frequency, the resemblances between low tones and beats, and binaural localization in terms of phase. The theory brings together in meaningful relation the evidence obtained in the study of the electrical responses of the ear, and presents a consistent account of the various phenomena of hearing.

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PROGRESS REPORT OF THE COMMITTEE ON DISPLACED FOREIGN PSYCHOLOGISTS

BY BARBARA S. BURKS

Chairman

Following a vote at the business meeting of the American Psychological Association in September, a Committee was appointed by the incoming President, Professor Gordon W. Allport, to survey the problem of foreign psychologists whose livelihood has been taken away and whose safety has been endangered.   The Committee, which provides regional representation in the United States and Canada (with a heavier concentration in the eastern states, where refugee psychologists are actually arriving in considerable numbers), consists of the following members:

Dr. Luton Ackerson, Institute for Juvenile Research, Chicago.
Prof. G. W. Allport, Harvard University.
Prof. W. E. Blatz, University of Toronto.
Dr. Barbara S. Burks, Carnegie Institution, Cold Spring Harbor, L. I.
Prof. D. B. Klein, University of Texas.
Prof. Gardner Murphy, Columbia University.
Prof. A. T. Poffenberger, Columbia University.
Dean G. D. Stoddard, University of Iowa.
Prof. E. C. Tolman, University of California.
Prof. Max Wertheimer, New School for Social Research, New York City.

Dr. Guenther Stern is an invaluable collaborator of the Committee. The aims of the Committee are to ascertain:

The present status, training, and qualifications of foreign psychologists who have been displaced from their positions through political developments of the past 5 years.

The possibilities of utilizing and conserving the potential contributions of these scholars to academic, social, industrial, and other institutions.

The points of view of the foreign scholars themselves as to how such contributions might be best effected.

To this end, members of the Committee have been collecting, and filing in the office of the Chairman, *curricula vitae* and relevant information concerning every psychologist who is an actual or potential refugee, and who could be reached by the Committee or by coöperating organizations. As it soon became evident that other committees were not specifically addressing the problem of scholars from other disciplines who have prominent psychological background and interests, the A.P.A. Committee also accepted for consideration a number of displaced scholars who do not belong exclusively to the field of psychology.

The Society for the Protection of Science and Learning (British) estimates in its November, 1938, report that " the total number of teachers

and research workers displaced since May, 1933, from German universities and institutions of university rank is now approximately 1400." An additional 400 have been displaced from Austrian institutions of equivalent rank since "Anschluss" in March. Although the group with which our Committee is directly concerned constitutes only a fraction of the total group from all the disciplines, it may be valuable to view our problem against a broad canvas. The following statistics were taken from the November report just cited:

GERMAN SCHOLARS (exclusive of Austria)

Total Displaced	1400	(approx.)
Permanently Established Elsewhere	520	
Temporarily Placed	290	
Unplaced	600	(approx.)

It is seen that not many over one-third of the displaced scholars have found a semblance of permanent security, and that over 40% are "unplaced."

These figures take into account those scholars who were attached to universities or institutions of similar rank. Speaking now of the psychologists, we have a comparable group of about 50 persons, and also a number of highly trained individuals whose services have been rendered in extra-academic channels, who do not appear on the published lists of displaced scholars, but whose situation is fully as distressing as that of the academic group. Although our survey is not yet complete, and is far from complete for the non-German countries, comparative figures upon 78 psychologists for whom we have fairly adequate information have considerable interest.

Of 28 psychologists who have lectured or directed research in institutions of university rank, a *few* are adequately placed in various countries; 13 have found at least temporary placement (often very inadequate placement); 15 are unplaced.

Of 24 psychologists representing the "applied field" (including secondary school teaching and guidance), 5 have found at least temporary placement and 19 are unplaced. Selection may operate in this "applied" group, in that unplaced psychologists may be more often known to members of our Committee than those who are placed and no longer in need of advice and aid; our lists of academic psychologists, many of whom come to us through organized agencies, are undoubtedly more complete than those of the less systematically canvassed applied psychologists. It is the impression of the writer, however, that during the first difficult period in a new country, the relatively protected milieu of a higher institution of learning can be more readily adjusted to than that of clinics, industries, etc., where knowledge of the language and of the *mores* counts more heavily. Moreover, the academic psychologists are freer to emigrate than are those of the other group, who must wait for a "quota" visa.

In addition to these 2 groups of academic and applied psychologists, we have a group of 26 young university-trained psychologists who had been assistants, or who had not yet become established in professional

work before conditions intervened to make their "habilitation" impossible. Of these, 9 are placed and 17 unplaced. Those who have come to our attention probably represent only a small proportion of psychologists actually falling under this heading. Several student psychologists have also been referred to us for advice as to how they could continue their studies—probably again only a small proportion of those whose studies have actually been discontinued.

We know of 2 psychologists who are in concentration camps at the present writing.

About 20 medical men, a dozen philosophers, and several scholars from related fields have been referred to the Committee—mainly men with research interests closely allied to psychology. Nearly all of these scholars are unplaced, or placed in unsatisfactory temporary positions. As the Committee did not originally attempt to survey these borderfields, it is safe to assume that these cases were referred to us chiefly in the hope of securing aid in placement.

The Committee is about to issue a list (with identifying data) of displaced psychologists and scholars in related fields. This will be sent for confidential use to universities and colleges and to other institutions on request.

With the task of "surveying the extent of the problem" partly but not wholly completed, and with a *few* scholars routed into positions through the efforts of its members (*e.g.* Professor Karl Buehler), the Committee now prepares to survey opportunities for placement. Bearing in mind the serious unemployment situation among American psychologists, the Committee is investigating the possibilities of "noncompetitive" openings, *i.e.* positions which may be newly created through the efforts of local academic committees which seek to assist their distressed colleagues, at the same time enriching the psychological thought of their own institutions; positions in regions or in countries where there is a visible undersupply of psychologists; positions in schools, social agencies, and industries in which the potential contributions of psychologists have only begun to be felt.

Funds for aid of the Committee's work to date have been made available through a loan from the National Coördinating Committee for Aid to Refugees and Emigrants Coming from Germany, through a grant from the Council of the American Psychological Association, and (mainly) from voluntary contributions.

Members and Associates of the A.P.A. are asked to communicate with the writer at Box 234, Cold Spring Harbor, Long Island, New York, if they have information on displaced foreign psychologists to file with the Committee. They are also urged to report openings, especially of a noncompetitive order, into which one or more of the refugees on our list might fit.

PSYCHOLOGICAL FACTORS IN MARITAL HAPPINESS

I.

TERMAN, L. M., assisted by P. Buttenwieser, L. W. Ferguson, W. B. Johnson, & D. P. Wilson. Psychological factors in marital happiness. New York: McGraw-Hill, 1938. Pp. xiv+474.

This volume sets out with the express intention of discovering the *correlates* of marital happiness. The presentation is characterized throughout by a strong sense of sin. Actually, the authors hope to find *determiners* of, *factors* in, *causes* of, and items *making for* said state, as well as an "adjustment scale" with predictive value. Periodically they urge the reader to remember that mere correlates, if any, have no causal implications; whereupon they feel free to indulge in exclamatory conclusions concerning the causal effects and influences of such correlates. But what is sauce for the reader ought also to be sauce for the authors. The procedure followed is that of the pious man who hopes to win salvation on the installment plan, interspersing spasms of wickedness between his recurrent confessionals.

Chapter 1 is properly cautious, defining happiness in operational terms as score on a specified questionnaire, and suitable reservations are made from extending the conclusions to different populations, times, and cultures. The more crucial question is the extent to which the inferences drawn actually apply to the population studied.

Chapter 2 presents the results of an exploratory search on 341 married and 109 divorced pairs, who submitted to modified Bernreuter and Strong questionnaires. General scores and detailed item reports were compared with score on a series of items calculated to yield a marital happiness index. The general trait scores showed little relationship, but some detailed items showed correlations that were felt to justify their later employment.

Immediately the causal question is raised: "Does marked husband-wife resemblance in these traits *make for* (italics mine) marital happiness?" And instead of "happiness results in one type of answers" we are told that "one type of mating favors happiness"; and this even when the traits are not measured until the marriage is of long standing, the divorce secured.

The correlation coefficients and other indices resulting from the "exploratory search" would have promptly brought to a halt less determined investigators. Table 2, for example, shows only 2 correlations (.20 and —.22) out of 48 that are 3 times their standard errors. But enthusiastic conclusions, terminating with exclamation marks, are found, such as (p. 23): "And how good a risk is the dominant male? The answer is that dominance in the husband correlates slightly positively with his own happiness but not with his wife's!" But the difference is

only that between coefficients of .14 and .04, when the standard errors are over .05 and the "dominance" is only a Bernreuter score!

On the whole it would have been better either not to have published the 35-page report of this preliminary search, or to have put it also in the Appendix. It creates a strong suspicion of the 400 pages to follow, especially when, later on (Chap. 8), the reader is frankly told that such correlations as those here eloquently commented on indicate "a degree of relationship that is almost negligible." If this volume ever gets into the hands of the newspaper boys they will get a lot of fun out of the solemn discovery that it is unfavorable for happiness for spouses to disagree on their liking for Pershing, that it is ominous for both mates to feel the same way about crossing the street to avoid meeting someone, and that among the best differentiating items of happy from unhappy spouses are mutual interest in dental work and joint fondness for men who use perfume. Thus low have the personality traits fallen.

But we have only just begun, and the report of the main investigation starts out no less loosely. On page 46 "the purpose is to identify *correlates* of the happy marriage." But the next chapter, 2 pages later, begins, "On what psychological traits does marital happiness *depend* (italics mine)?" The data are the checked replies on a 12-page questionnaire by 792 willing couples, married on the average 11.4 years, and reasonably representative, it is thought, of the middle to upper-middle occupational, economic, and cultural levels of the urban population in California. However, the mean educational level was college junior and 674 of the individuals were college graduates or better.

Chapter 3 describes this fuller investigation, the methods of collecting the couples and securing anonymity, the information schedules, and the general plan of the investigation. Happiness indices for each individual were derived by totaling weighted scores from replies to questions about common interests, disagreements and their settlement, attitudes toward the marriage and the spouse, domestic grievances, and subjective estimates of happiness. These scores were markedly skewed in their distribution, piling up at the happiness end. The questionnaire contained, also, 3 other sections. One contained 233 "personality test items" (likes, interests, attitudes, response habits, opinions). Another contained questions concerning personal history, family, training, and background. The last set of items dealt specifically with habits and characteristics of sex experience and intercourse.

Chapter 4 is devoted to an analysis of the happiness scale and the interrelationships of the items used in deriving it. Chapter 5 undertakes to interpret the strikingly differential "domestic grievances" when happier and less happy marriages are compared.

At this point a serious methodological fault is encountered. Large critical ratios resulted when the grievance scores of happier and less happy individuals were compared—higher indeed than those found with other sections of the questionnaire. But the "interpretation," partly ex-cathedra and partly supported by internal contradictions, discards these high "correlates" on the ground that they are not causes but consequences. They are felt to be colored by the "halo"

and "rationalization" due to marital mood. "Unhappy spouses display an irrestible (sic!) inclination to account for their unhappiness in terms of almost anything that is suggested" (p. 314).

There is no question in the reviewer's mind that this interpretation is sound. But, once admitted, it vitiates the whole procedure of the investigation. For the "halo" thus demonstrated infects also the replies to items of "personality," "back-ground," and specific sexual habits as well. There is evidence for such an outcome, besides mere assertion, in the literature of questionnaire technique. Nearly 20 years ago the effect of the Armistice was shown on the replies by invalid soldiers to an inventory not unlike those used in this study. The Armistice was found to produce a notable change in the report of neurotic and other complaints, amounting sometimes to a miraculous cure. Paraphrasing the present volume we could say of these results: "Unhappy soldiers display an inclination to account for their unhappiness in terms of almost anything that is suggested."

But the Armistice mood colored not only the report of personal complaints; it also strikingly modified the reports of family history and background. Quoting from the initial report of these findings:

"The present condition of patients became enormously improved. Not only is this true, but even the facts of their history are more optimistically reported, and intermediate facts are intermediately colored. . . Before the Armistice the general disposition toward invalidism inclined the reports toward unfavorable perversion and pessimistic interpretation. After the Armistice the general urge toward release from military service led in much the same way, and probably to much the same degree, toward a favorable distortion of biography, heredity and experience, and a more optimistic interpretation than the facts would warrant in a report not definitely biased by motivation."

The analogy between marital ties and the bonds of military service is more than superficial. In the absence of clear evidence to the contrary it is fair to assume that marital and martial moods act in much the same way. If "the approach which relies upon direct questioning of the subject about his grievances can be and perhaps usually is grossly misleading" (p. 109), then it is doubly distressing that the personality indicators and case history data utilized in the 5 chapters to follow in the search for "causal factors" were secured in the same way. Not only are they mere "correlates," but they also may be expected to be "colored" correlates, perhaps just as "colored" as the domestic grievances. Even the combined "estimates of three psychologists" (p. 119) can do nothing to mitigate this misfortune. After all, it might have been safer and cheaper to rest with the main "theory," advanced in this chapter and later reaffirmed, that happy marriages are those made by happy people.

The ratios of specific complaints as between happier and less happy individuals are taken not as measures of any causal influences but merely as "rationalization indices," and it is declared that "viewed as factual testimony, the responses to the grievance items are next to worthless" (p. 95). In the light of this declaration the value of any other item reported in the questionnaire becomes infected with grave suspicion.

If the ratios of complaints are "too large to be accepted as credible evidence of fact," who is to decide when a ratio is to be accepted? If some data secured by a given questionnaire are to be dismissed as "incredible," what excuse is there for believing anything in those records? Surely, only an *obiter dictum,* such as has no place in a "metric" study. At the very least, some individual index, such as a "rationalization score," might have been used, which would probably come closer to being a revelation of "personality" than do many of the piddling items actually inquired about and given a "prediction weight."

Having shown that expressed grievances are "merely symptomatic," the authors turn in Chapter 6 to what they blithely call "factual information about the differences between happy and unhappy spouses with respect to their personality characteristics, their life histories, and their general background." Although, as we have seen, there is reason to suppose they have few or no such facts, still there are the answers to the questionnaire. "Happier" and "less happy" criterion groups were formed, comprising 300 and 150 of the total 792 couples, respectively. Based on comparison of the replies from these groups, and guided by the best judgment of 3 psychologists who tried to eliminate items estimated to be effects rather than causes, each item was assigned an appropriate, though arbitrary, weight as a component of a relevant personality scale. In Chapter 7 the personalities of happier and less happy mates are portrayed in various ways, using 132 differential items from the Bernreuter, Strong, and "opinion" lists. If the logical choice of items has any validity at all, then the inquiry itself was needless. If it has no validity, or only an indeterminate one, then the portraits given are equivocal. Moreover, they are usually in terms of probable trivialities, and at best are "ex post facto"—drawn after an average of over 11 years of marital endurance.

Chapters 8, 9, and 10 deal with "background" factors. The data are the replies to questionnaire items, with a value that is indeterminate because of just such halo influences as led war invalids to falsify their family histories. If factual data were available, these replies might be used to indicate *which* reports of background will be most notably colored by marital mood.

However, considering the items as reported, no significant correlations were found between happiness scores and income, occupation, children, religious training, birth order, sibling relationships, adolescent popularity, age differences, or schooling differences. Numerous items showed a small correlation, and are given small prediction weights.

Most related to marital happiness are parent happiness, childhood happiness, lack of conflict with parents, firm but not hard discipline, attachment to parents, parental frankness about sex, mild or infrequent punishment, lack of premarital aversion toward sex. These are given heavy prediction weights. It must be noted that these are precisely the kind of background items that could most easily be colored in memory by current mood. They represent mainly "complaints about parents" rather than "complaints against spouse." In fact, the rank for predictive value is probably the same as that for susceptibility to the "halo" effect.

Childhood happiness, for example, the authors admit is vulnerable to this error. Yet it is assigned high predictive and causal importance. It is even implied that marital happiness could be promoted by "making childhood happiness a universal birthright."

Chapters 11 and 12 consider the third main topic, namely, specific sexual adjustments, habits, practices, and techniques. The data from this section of the questionnaire are not called "personality" items. To the reviewer they appear to be at least more intimately *personal* than interest in Pershing, in dental work, or in pet canaries. The items cover a variety of questions relating to frequency, duration, enjoyment, time, and technique of intercourse, contraceptive practices, relative passion, orgasm adequacy, premarital and extramarital intercourse and inclinations, homosexual tendency, sleeping arrangements, etc.

Most of these, contrary to the sexologists, show no correlation with marital happiness. Some of the statistical findings would be of intrinsic interest if there were any way of knowing how representative these willing and patient married pairs were, even of their own culture group. The graphs picturing the approaching extinction of virginity at marriage, in both sexes, are an example.

Some of the items show sufficiently high critical ratios to be assigned weights in the prediction scale—though by no means proportionate to their differential values. Sexual complaints of spouse and sleeping in separate rooms, for example, are dismissed because "our own interpretation is" that they are results or symptoms rather than causes. Orgasm adequacy of wife and husband-wife equality or near equality in sex zeal give high correlations with happiness, and these, it is concluded, are "determiners." It is this mixture of ex-cathedra decision (however wise) with statistical findings, in which the wisdom always overrules the critical ratios, that the scientific reader will most regret.

Chapter 13 surveys the relative "contributions" ("correlates" has now been dropped) of personality, background, and specific sexual factors to marital happiness. It is maintained that the first 2 sets of scores, secured *before* the marriage, would be the same as when, 11.4 years later, "rationalization" and "halo" have done their work. Of this, as we have intimated, there is grave doubt.

The difficulty in determining what, if anything, these questionnaire returns mean, is illustrated by the way the authors wrestle with "infidelity of wife." The "opinion" data show this to be reported as undesirable or intolerable in over 95% of the cases. But the complaint lists show it to be less serious for husband's happiness than "not having meals on time." The opinion vote, it is felt, "may have been a parrotting of the traditional ideal rather than an expression of the real beliefs." And the grievances, as we have seen, although they have high discrimination values, have no real significance, because of the "halo." That is to say, the meaning of marital infidelity, if and when it should *really occur*, is wholly untouched by this investigation. So, the reviewer believes, is everything else. But this, one fears, will be overlooked by the feature writers, the teachers of marital adjustment courses, and the popular summaries in the pulp magazines.

The main point stressed is that these personality and background factors are as closely related to happiness as are the more strictly sexual scores. Arguments and some experimental results are given for the predictive use of the adjustment scale and comparison of its validity is made with that of other psychological techniques of prediction in this and other fields. Here also it is admitted that there is much overlap between what have been up to this point called personality factors and the reports on specific sexual items.

The last chapter gives a lucid summary and general conclusions. Four appendices give detailed analysis of the correlates of orgasm inadequacy, which remains a " mystery "; data comparing various special groups; percentile norms for the main portions of the scale; and the information schedule in full. There are 28 illustrative graphs, 147 tables, a bibliography of 114 relevant titles, and name and subject indices.

The volume contains a wealth of detailed analyses, correlations, differentiating values, critical ratios, and other comparisons, involving all the items here mentioned, and many others. It is full of instructive comparisons of these results with such conclusions as others have announced, with interesting, even if subjective, considerations of various possible interpretations of the results. The reader will find the discussion more interesting than this review might lead him to expect. Whatever its adequacy, the study is particularly timely in view of the current interest in instruction for marital adjustment and it will afford a mine of concrete material to replace the usual wordy content of such guidance courses. One trembles to think of the amount of probably misguided advice that will be administered on the basis of these provisional conclusions by people who miss the total picture and pin their faith to the details.

The prestige of the authorship and sponsorship will give this volume a halo of authenticity that will put its tentative intimations into the fireside journals as the final conclusions of science, and the science will be psychology, which already has a hard enough job defending its precarious status.

Finally, the main impression left by the book is that the authors are convinced that sexological, physical, and medical factors are less important determiners of marital compatibility than they have been said to be by the armchair authorities. The most important single determiner is surmised to be a psychological one, viz., temperamental and, probably in great measure, constitutional proclivity for happiness on the part of those entering into the relationship.

The authors acknowledge the tentative character of their results but emphasize the importance of further research in this field by such psychometric techniques as they have used. They are confident that " within a decade or two it will be possible by such methods to predict marital compatibility with greater accuracy than it is now possible to predict academic success from a composite of intelligence test score and previous scholarship records."

It might be suggested, as an afterthought, that the practical question is not how the happiness of one pair of spouses compares with that of another, but, instead, whether a given individual would (1) be happier

married than single, and (2) which other individual, if any, he or she could most happily wed. On such issues the present results appear to shed no light. They are all *ex post facto.*

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<div align="center">II.</div>

THE EFFECT OF HAPPINESS OR UNHAPPINESS ON SELF-REPORT REGARDING ATTITUDES, REACTION PATTERNS, AND FACTS OF PERSONAL HISTORY

<div align="center">BY LEWIS M. TERMAN</div>

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It has long been a matter of common knowledge that many varieties of personality-test items are invalid if the respondents know the purpose of the test and have something to gain by making themselves appear to be other than they are. The question has been raised whether the halo of domestic happiness or unhappiness of married subjects similarly invalidates self-report regarding attitudes, reaction patterns, and facts of personal history when the subjects not only have nothing to gain by coloring their reports but know in advance that neither the investigator nor anyone else can possibly find out how any respondent has answered a particular question. Light on this question is available from 2 sources.

In the first place, as is stated in my book on marital happiness (pp. 357–358), a partial check was made by comparing the correlations of personality-background factors with marital happiness separately for 2 groups so matched as to rule out irrelevant variables: (1) a group married 2 to 5 years and (2) a group married 15 years or longer. The point of this comparison is that, if responses of subjects to the personality-background items are seriously affected by the happiness or unhappiness which has developed in the marriage, these personality and background reports should correlate more highly with happiness scores in the long-married group than in the briefly-married group. The trend of the correlations was, in fact, slightly in the opposite direction. This finding suggests that responses to such items secured at the time of marriage, or even prior thereto, will yield similar correlations with later marital happiness.

More crucial evidence is furnished by a comparison of the responses made by 134 subjects in the post-honeymoon period (married less than 24 months) with those made by 284 subjects who had been married 6 to 8 years. We may, for the present, designate these groups as "A" and " C," respectively. As would be expected, the "A" group has developed little unhappiness. In fact only 6 of the 134 subjects in this group admitted that they were unhappy. The " C " group, on the other hand, had the lowest mean happiness score found at any interval for length of

marriage. In the "A" group only 8, or 6.0%, have happiness scores below 55, as compared with 61 subjects, or 21.5%, in the " C " group. The difference is 5.2 times its standard error. Above the happiness score of 74 there are 65.9% of the "A" group and 45.1% of the " C " group. This difference is 3.8 times its standard error. So great is the contrast between the 2 groups with respect to happiness that we are justified in designating one as the " before " group, the other as the " after " group, meaning before or after unhappiness has had time to develop.

If self-reports by married subjects are greatly influenced by the halo effects of existing happiness or unhappiness, the responses of the " after " group on personality and background items should differ greatly from those of the " before " group. Such is not the case. Instead, as will be seen in Tables I and II, the distributions of responses by the 2 groups are practically identical. The " before " group, which had not had time to distribute itself widely on the happiness scale, answered the questions neither more nor less optimistically than the " after " group.

The 9 background items in Table I include those carrying the higher weights and account for about three-fourths of the total background score for prediction of marital happiness. The 17 personality items of Table II include 12 that carried weights of 2 points for one or both spouses (these being the heavier weights), and 5 that had been assigned reduced weights because of possible doubt as to direction of the cause-and-effect relationship. Both the background and the personality items chosen for this comparison are precisely those that would be most likely to be invalidated by halo effects of happiness or unhappiness.

On several of the items in Tables I and II outside evidence could be cited in support of my data. Consider, for example, the correlation between the marital happiness scores of my subjects and the rated marital happiness of their parents. Is this correlation an artifact produced by halo effects upon report, or is it true that marital happiness and unhappiness tend to run in families? In an attempt to answer this question Popenoe and Wicks[1] had the happiness of more than 2000 marriages rated not by the spouses themselves but by a third party who at the same time rated the marital happiness of the parents of the spouses. From their table of results I find the tetrachoric correlation to be .41 between happiness of the husband and of his parents, and .31 between happiness of the wife and of her parents. These correlations are higher than those yielded by my data (.25 for husband and .21 for wife). Comparing their findings with those of 2 earlier studies, the authors conclude that " these three studies point in the same direction and leave no doubt that happiness runs in some families, unhappiness in others."

Evidence to the same effect is available from my 16-year follow-up of more than 1200 " gifted children " in California. Up to about a year ago, 56 of the 608 marriages contracted by members of this group had ended in separation or divorce. Of these 56 subjects, 21.0% had parents who were separated or divorced. Of the 552 subjects in unbroken mar-

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[1] P. Popenoe & D. Wicks. Marital happiness in two generations. *Ment. Hyg., N. Y.*, 1937, **21**, 218–223.

TABLE I

COMPARISON OF "BEFORE" AND "AFTER" GROUPS ON RESPONSE TO BACKGROUND ITEMS

	"Before" Group (N=Approx. 130) %	"After" Group (N=Approx. 280) %	Diff. $\overline{\sigma \text{ Diff.}}$
1. Rated happiness of parents			
Above "average"	43.0	42.4	...
"Average" or below	57.0	57.6	...
2. Rated conflict with father			
Less than "average"	71.2	68.3	...
"Moderate" or more	28.8	31.7	...
3. Rated conflict with mother			
Less than "moderate"	74.6	71.4	...
"Moderate" or more	25.4	28.6	...
4. Rated attachment to father			
"Moderate" or less	41.1	47.0	1.1
More than "moderate"	58.9	53.0	1.1
5. Rated attachment to mother			
"Moderate" or less	37.1	43.8	1.3
More than "moderate"	62.9	56.2	1.3
6. Rated happiness of childhood			
"Average" or above	84.8	84.4	...
Below "average"	15.2	15.6	...
7. Childhood discipline			
"Strict" or "firm but not harsh"	63.3	68.8	1.1
Lax or irregular	36.7	31.2	1.1
8. Childhood punishment			
"Severely" or "frequently"	20.4	23.1	...
"Occasionally," "rarely," or "never"	79.6	76.9	...
9. Parents' attitude toward child's early sex curiosity			
Answered "frankly" or "briefly"	49.3	47.5	...
Lied, rebuffed, or punished	50.7	52.5	...

TABLE II

COMPARISON OF "BEFORE" AND "AFTER" GROUPS ON RESPONSE TO PERSONALITY ITEMS

	"Before" Group (N=Approx. 130) %	"After" Group (N=Approx. 280) %	Diff. $\overline{\sigma \text{ Diff.}}$
1. Often feel just miserable?			
Response *No*	56.0	63.9	1.5
" *Yes* or *?*	44.0	36.1	1.5
2. Frequently feel grouchy?			
Response *No*	57.4	60.5	...
" *Yes* or *?*	42.6	39.5	...
3. Frequently feel lonesome when with other people?			
Response *No*	64.2	65.0	...
" *Yes* or *?*	35.8	35.0	...

TABLE II—Continued

	"Before" Group (N=Approx. 130) %	"After" Group (N=Approx. 280) %	Diff. σ Diff.
4. Often in a state of excitement?			
Response *No*	66.4	65.6	...
" *Yes* or *?*	33.6	34.2	...
5. Harder to be serene and cheerful than for most people?			
Response *No*	83.6	79.0	1.2
" *Yes* or *?*	16.4	21.0	1.2
6. Try to get your own way even if you have to fight for it?			
Response *No*	49.2	53.4	...
" *Yes* or *?*	50.8	46.6	...
7. Touchy on various subjects?			
Response *No*	51.5	51.9	...
" *Yes* or *?*	48.5	48.1	...
8. Useless thoughts keep coming into your mind to bother you?			
Response *No*	59.7	65.1	1.0
" *Yes* or *?*	40.3	34.9	1.0
9. Usually avoid asking advice?			
Response *No*	56.7	59.8	...
" *Yes* or *?*	43.3	40.2	...
10. Feelings alternate between happiness and sadness without apparent cause?			
Response *No*	53.8	61.3	1.4
" *Yes* or *?*	46.2	38.7	1.4
11. Critical of other people?			
Response *No*	47.7	50.7	...
" *Yes* or *?*	52.3	49.3	...
12. Does discipline make you discontented?			
Response *No*	51.5	54.2	...
" *Yes* or *?*	48.5	45.8	...
13. Feelings easily hurt?			
Response *No*	47.8	52.2	...
" *Yes* or *?*	52.2	47.8	...
14. Avoid saying things that might hurt anyone's feelings?			
Response *No* or *?*	28.4	27.5	...
" *Yes*	71.6	72.5	...
15. Disregard feelings of others when accomplishing an end important to you?			
Response *Yes*	30.6	27.1	...
" *No* or *?*	69.4	72.9	...
16. Lose your temper easily?			
Response *No*	76.9	71.1	1.3
" *Yes* or *?*	23.1	28.9	1.3
17. With opposite sex tend to be dominant and have own way?			
Response *No*	65.7	67.7	...
" *Yes* or *?*	34.3	32.3	...

riages, only 11.8% had parents who were separated or divorced, or little more than half as large a proportion.

Finally, in my marriage study I have compared ratings of parental happiness given by the entire group of subjects with the similar ratings by 72 of the subjects who had been married and divorced prior to the present marriage. In this comparison I have included only the previously divorced subjects whose spouses had not been married and divorced. We thus have a 2-directional comparison: (a) between parental happiness of previously divorced persons and parental happiness of the entire group (90% of them in their first marriage); (b) between parental happiness of previously divorced persons and parental happiness of their spouses (who had not contracted a previous marriage). The results are shown in Table III.

TABLE III

PARENTAL HAPPINESS RATINGS BY TOTAL MARRIED GROUP, BY SUBJECTS
PREVIOUSLY MARRIED AND DIVORCED, AND BY SPOUSES
OF SUBJECTS PREVIOUSLY DIVORCED

	Total Group %	Divorced Group %	Spouses of Divorced Group %
Rated Happiness of Parents			
Above " average "	48.6	37.5	50.0
" Average " or below	51.4	62.5	50.0

It will be noted that, whereas the per cents in column 3 are almost identical with those in column 1, the per cents in column 2 diverge markedly from those in column 1. The latter difference is approximately twice its standard error. Again we find that marital happiness and unhappiness tend to run in families. Halo effects have not appreciably affected the data on this question, as reported in my book. If the reader examines the text he will find a considerable amount of outside evidence cited on various other items relating to the personal history of the subjects.

The almost complete lack of halo effects in responses to personality and background items is in striking contrast to the presence of such effects in responses to items that call for specific complaints against the spouse or the marriage. In the latter, as I have shown, halo is present to such a degree that the complaints registered must be interpreted, for the most part, as rationalizations of the existing state of marital unhappiness.

Crucial information on the validity of the personality and background items I have used in the prediction of marital happiness scores is fortunately becoming available from a parallel investigation being carried out by Dr. Lowell Kelly under the same sponsorship that made possible my own. Dr. Kelly has applied a great number of physical and psychological measures to some 300 engaged and newly married couples and is charting, year by year, the course of each marital ship. For the sake of comparability of results he has secured from his subjects responses to

the same questions on personality, background, and happiness that the Stanford study has utilized.[2] At my suggestion he has been kind enough to examine his material for such evidence as is now available regarding the validity of my happiness prediction scale, and to summarize this evidence in the note which follows.

III.

CONCERNING THE VALIDITY OF TERMAN'S WEIGHTS FOR PREDICTING MARITAL HAPPINESS

BY E. LOWELL KELLY

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As Dr. Terman has stated, he and I have been conducting simultaneous and somewhat parallel studies [1] of psychological factors underlying marital compatibility. The essential difference between the 2 investigations is that Terman's was cross-sectional, whereas mine is longitudinal or genetic. Terman obtained his information from married couples who were contacted but once *after* marriage. Almost all of the 300 couples being studied by me were interviewed and were administered tests *before* marriage. The success of the marriages is being evaluated by means of an annual follow-up blank similar to the scale of marital happiness used by Terman.

The personal-interview situation permitted the collection of considerably more data than was possible in Terman's investigation, but the long-time nature of my study makes it impossible to check the validity of the individual items until more time has elapsed. Fortunately, however, all of the items used by Terman were included in my study, and a preliminary check of the validity of his prediction weights is possible at this time.

At Dr. Terman's suggestion I have sorted out my distribution of responses to certain background items for the 300 couples who filled out my marriage schedule before marriage or very shortly thereafter. These include: happiness of parents, conflict with father, conflict with mother, attachment to father, attachment to mother, happiness of childhood, and childhood punishment.

Of the 7 items, 2 give distributions almost identical with Terman's. For the other 5 items the distributions, while not differing greatly from Terman's in the absolute sense, diverge enough from his to make the difference statistically reliable because of the rather large N's in the

populations compared (approximately 280 and 580). The divergences could easily be due to cultural differences between New England and California, or to selective factors operative in " willingness to coöperate " in the 2 studies. One of the 5 reliable differences (conflict with mother) is, in direction, contrary to the reviewer's criticism.

Much more pertinent is the question whether Terman's prediction measure actually predicts happiness for my subjects who answered the questions before there was any possibility of marital unhappiness being present. I have computed the total prediction score (personality plus background) for 82 of my couples who have been married 2 years, and I have computed for these couples the correlation of the prediction score with marital happiness score at the end of 2 years. The resulting raw correlations are .26 for husbands and .30 for wives. When adjusted to allow for the extremely narrow happiness range of my group, these become .50 for husbands and .56 for wives. The corresponding correlations reported by Terman for 200 couples were .54 for husbands and .47 for wives.

In spite of the theoretical objections to assigning " prediction " weights on the basis of mere " correlation," Terman's weights seem to be valid for an entirely different population, which responded to the items before marriage.

BOOK REVIEWS

THURSTONE, L. L. Primary mental abilities. Chicago: Univ. Chicago Press, 1938. Pp. 121.

This monograph is the first of the series sponsored by the Psychometric Society. The author's purpose is to " describe the fundamental ideas of factor analysis in terms of only the simplest mathematical ideas in the belief that some readers who are not interested in the mathematical proofs of the previous volume (*The vectors of mind*) may, nevertheless, have some interest to know the essential nature of these methods." In this he succeeds quite well, with the single exception of Chapter IV, which is probably still beyond the average ability of those uninitiated in factorial mathematics. The general effect of reading this book after laboring through *The vectors of mind* is very similar to the rather satisfying experience one has upon looking at the completed structure after having toiled through the blueprints.

The general purpose of factorial analysis is to describe succinctly and adequately the individual differences in human abilities by finding the several primary abilities which underlie human behavior. The necessity for such an analysis is dictated not only by the economy of effort and attention which the results would provide, but also by the deeper understanding which it would give of all human abilities with special relationship to the influence of genetic factors as well as cultural factors.

Ever since it became apparent that factorial analysis does not lead to a unique result, but that there were an infinite number of results possible, workers in this field have devoted their attention to finding some criteria for selecting one result rather than another. The criteria that have been proposed thus far are: invariance of results, psychological interpretation of results, and practical utility of results.

In order that the results be invariant, the factorial structure of a given test must remain constant from one analysis to another. This is often not the case in the results obtained thus far. Sometimes the introduction of a new test or the removal of an old test changed the factor loadings (*i.e.* correlations between tests and factors). These correlations were also found to depend upon the sample of individuals tested. It is clear that if the correlations between a given test and a factor are to be stable, they ought not to change from one situation to another, or, if they do, adequate explanations for such changes must be provided.

In order that the results may be psychologically meaningful, some consistent hypothesis must be available for explaining the nature of the factors. Tests that correlate highly with a given factor must have something in common which is psychologically understandable. Unless some psychological explanation of this common bond is available, the results cannot be regarded as meaningful. Then, also, negative factor loadings crop up in many results. How a factor can affect a test by subtracting from it rather than contributing to it is difficult to comprehend.

In order that the results have practical value the factor scores, *i.e.* the

relative amount of each factor possessed by a single individual, must be utilizable in the description of the individual's ability or behavior. This, in the final analysis, is the purpose of all measurement. It is not sufficient for the results to be psychologically meaningful in the sense that they are explicable in terms of some hypothesis. The factor scores should also serve the same end as do test scores, but should serve it in a much better way. Unless the factor scores turn out to be superior to the original test scores, why spend all the time and effort in factor analysis?

The first two conditions, invariance and meaningfulness of the results, are declared by Thurstone to be dependent upon our ability to find the simplest factorial structures for the test under analysis. This simple structure is to be obtained by first analyzing the correlation table by means of the centroid method and then rotating the axes of the obtained factors graphically until the number of zero factor loadings for the tests (*i.e.* the number of tests that are independent of each factor) is maximized and the number and size of negative correlations with each factor are minimized. The reason for wishing to minimize the number of negative correlations is obvious. The purpose of maximizing the number of zero correlations needs more examination.

It is to be expected, Thurstone points out, that some tests will have zero correlations with several of the factors, for it is probably impossible to create a battery of tests each of which will encompass all the psychological factors that may be operative. Taking this as his cue, he rotates the axes for each pair of factors in such fashion that the number of zero loadings is maximized. In other words, each test can be regarded as made up of as few factors as possible. One can readily follow Thurstone in his first statement, but his conclusion that the number of zero loadings must be maximized does not seem to follow as a necessary result. Furthermore, the assumption that a simplified structure will remain more invariant than any other type of structure is an assumption that requires more empirical proof than is now available. But even granting that Thurstone's simplified structure will render the factor matrix independent of the removal or addition of any single test, the factorial structure may yet be altered by other means. One of these is the characteristics of the individuals in the sample. Thurstone pays little attention to this source of possible invariance, although there are studies pointing in that direction.

One necessary result of the rotation of axes and the maximizing of the number of zero factor loadings is the elimination of the general factor running through all the tests which Spearman postulates. It is therefore somewhat difficult to follow Thurstone's conclusion: " So far we have not found the general factor of Spearman, but our methods do not preclude it." If conscious rotation to eliminate the general factor does not preclude it, what will?

The third condition, that of practical usefulness, is dealt with in the chapter on the interpretation of the factors. In this section Thurstone gives the net results of the application of the factorial analysis to the fifty-six tests, the work having taken several years. This chapter reveals most enlighteningly the long road that factor analysis still has to travel

before it becomes useful to the practical psychologist. Perhaps the most difficult problem of all is the problem of describing the factors after they are isolated. The usual procedure is to take some arbitrary factor correlation like .40 (the limit used by Thurstone) as the critical value. Tests that do not correlate with the factor to the extent of more than .40 are not considered in the determination of the character of the test. This procedure is, of course, highly arbitrary, and it is doubtful whether unanimous agreement could ever be reached on the proper name or content of a given factor from a knowledge of the tests with which it correlates. Indeed, Thurstone himself finds difficulty in explaining the presence of high factor loadings for some tests and their absence in the case of others. In one or two instances some unusual *tour de force* is resorted to in order to explain the factor loading of a test. For example, in order to explain the high visual or spatial factor loading of the syllogism test, the author has to resort to the following: " The essentially visual character of the syllogism test is readily seen by inspection. The premises are concerned about the relative ages of three individuals, and the simplest way to keep them in mind is to represent them by lines or relative elevations. This is the way most people solve these syllogisms, and hence facility in visualizing augments the score." It is at least questionable whether most individuals utilize this type of visual imagery in syllogistic reasoning. The procedure of inferring the nature of the factor from the tests with which it correlates is a hazardous task and is in great need of supplementation by more objective devices, if any can be found.

The technique, disappointing as it is from the point of view of immediate returns, nevertheless seems to hold promise for the future. The succinctness of the approach must command the respect of all of those who become acquainted with it. The rapidity with which improvements are being made and difficulties removed is also striking. One source of difficulty that has often confronted previous work in factor analysis was the question of where to stop in the determination of factors. There was no way of telling whether all the factors had been extracted. No mathematical solution of this problem is yet available but an empirical device for determining whether or not the number of factors is complete has been evolved.

The rotation of axes at the present time is conducted in a graphic, nonmathematical manner. It is hoped that within the next few years a more rigorous mathematical procedure for performing analytically the work which is now done graphically will become available.

Despite the rapid advance that factor analysis has made in recent years, there is still much left to be done. Thurstone realizes the basic inadequacies of the present status of factorial procedure and in some instances is quick to point out these difficulties as well as possible remedial methods for the future. Perhaps one of the most outstanding difficulties is the question of the linearity of relationship between the original variables. Thurstone side-steps this issue completely by discarding the actual distribution surface and substituting for it a dichotomous distribution for each variable, the dichotomy being drawn at the line of the medians. This is done not only for the relationship between the variables

but also for the reliability of each test. The tetrachoric correlations that are obtained in this manner are, of course, dependent upon the normality of the distribution surface as well as its continuity and on the linearity of the regression line. Whether these assumptions are at the basis of any of the difficulties that Thurstone finds in interpreting the results is well worth investigating.

A second difficulty that Thurstone recognizes is the apparent non-significance of the individual profiles that emerge when the factor scores for each individual are determined. One wonders whether this result is not in part due to the above-mentioned artifact in the utilization of tetrachoric correlations. Perhaps a second reason for this result is the fact that the factors themselves are independent by definition. In other words, the coexistence of any combination of factor scores in a single individual is purely a matter of chance. Small wonder, then, that no definite patterns tend to appear. They are made impossible by the nature of the factor scores upon which they are based.

Some of the difficulty which is being experienced in factor analysis today might be obviated if, instead of proceeding in the direction of factorial analysis of traits alone, we would supplement it by factorial analysis of individuals; and, indeed, Thurstone points out a recent trend in that direction. However, those attempts, especially those of Stephenson and Burtt, are posited on a statistical basis which presents many difficulties. Stephenson and Burtt use as their starting point the correlation between pairs of individuals instead of the conventional correlation between pairs of tests. The correlation between two individuals considers each of the two individuals as a " test " or " trait " and the pairs of scores on each of the different tests as belonging to different " individuals." However, one of the assumptions of any distribution in a correlation surface is that the frequencies of the scores must be independent of each other. It is difficult to see how a score on one test can be independent of a score on another test made by the same individual, especially if the correlation between the two tests is significant. A second difficulty, which perhaps is inherent in the first, is the impossibility of finding a continuum on which to distribute the scores of the same individual on different traits. Still a third difficulty is the selection of the ideal for each type which is to serve as the basis of correlation. The selection of the ideal individual is, at best, arbitrary at the present time.

The success that Thurstone's approach has won thus far seems to be limited to the field of mental abilities of the intelligence test type. He admittedly finds little success in the field of personality and vocational guidance, and is pessimistic enough about the personality tests of the paper-and-pencil variety to state that they are doomed to extinction. He expresses similar opinions about group tests in general and indicates that the future belongs to individual tests. It is interesting to speculate on the causes of this failure in the field of personality tests. Perhaps one of the reasons of the lack of success is the empirical check that the clinical field affords. It makes very little difference at the present time, practically speaking, whether we regard a given test as made up of g or of s or of v. However, if some personality factors of an observable kind

are postulated in an analysis, it soon becomes apparent whether the factors correspond to observed behavior or not.

Perhaps a second reason for the failure in the field of personality is the inadequate standardization and validation of these tests. As long as most of these tests are validated in an *a priori*, logical manner rather than in an empirical way, one cannot hope to obtain psychologically meaningful results from them.

A third difficulty that lies at the basis of the failure is perhaps fundamental to the entire factorial approach. Factorial analysis is primarily an attempt in the classification of human abilities and, in the last analysis, will stand or fall on the degree of usefulness which the classification system will provide. The assumption is made that a frame of reference exists in which human abilities can be described in terms of a comparatively small number of continuous, normally distributed, independent factors. Perhaps such a frame of reference exists, but if it does, it defies some of the concepts now current in psychological thought. It is difficult to make such a system compatible with Gestalt theory or with typology. Independent factors cannot give rise to any specific patterns or structures in individuals since independence precludes anything but a chance relationship in the distribution of factor scores in a given individual. Gestalt and typology ought at least to be given a chance, and the present factorial attack precludes this chance. Perhaps by starting at the other end of the task and beginning with groups that are recognized as socially distinct, as, for example, delinquents and nondelinquents or successful and unsuccessful teachers, we may supplement Thurstone's approach and help crystallize the meaningfulness of the factors that have thus far been found. If, instead of throwing our full energy into the task of finding the primary abilities of man, we were to spend part of our time trying to discover the differential symptoms or syndromes of symptoms that distinguish contrasted socially recognizable groups, we might, as a result of the latter analysis, aid in the determination of the intrinsically primary abilities of man.

It is apparent that Thurstone has given us a very sharp tool which can cut keenly into our psychological problems, if used wisely. At present the tool is far sharper than it is useful. How to render it more useful is indeed one of the outstanding problems facing differential psychology. Perhaps by combining the contributions of differential psychology with those of typological psychology, both methods will be benefited and present-day psychology will be brought out of its chaotic condition.

JOSEPH ZUBIN.

New York State Psychiatric Institute and Hospital.

KALLMANN, F. J., with the assistance of S. J. Rypins (with an introduction by N. D. C. Lewis). The genetics of schizophrenia: a study of heredity and reproduction in the families of 1,087 schizophrenics. New York: Augustin, 1938. Pp. xvi+291.

The basic material of this study consists of all the schizophrenic case histories available in 1929 in the archives of the Herzberge Hospital in

Berlin from the first ten years of its existence (1893–1902). Four years of field work were devoted to the collection of reproductive and psychiatric data about the ancestors and descendants of the patients ("probands") and about their siblings and the children of the latter. The principal objects of the survey were to ascertain the level of fertility and mortality of the probands and their various descent groups, and the incidence of psychopathy among them; and, where control data (from other studies) existed, to make the comparisons. Secondary projects were a description of the ancestry of the probands, a determination of the incidence of psychopathy other than schizophrenia and " schizoidia " in the descent groups, and a study of the relationships between schizophrenia and tuberculosis. The work is furnished with the usual scholarly paraphernalia, of which a large bibliography is the most important.

It is hardly necessary to say that the work is executed with Teutonic thoroughness, and at once takes its place as one of the fundamental sources on the inheritance of psychopathy. The results are largely a matter of how you look at the problem. One way of looking at it, which we may call the eugenicist's view, is to focus attention on the expectation, in the statistical sense, of developing any form of psychopathy, given membership in a specified descent group; at the other extreme is what we may call the view of the common man, who wants to know the answer to such practical questions as whether his children by the niece of a schizophrenic would have any substantial likelihood of incurring mental illness. It makes a big difference. The answer to the eugenicist's problem is that " types belonging to the schizophrenic disease-complex " have an expectation of 44% in the children of schizophrenics, 25% in their grandchildren, 3% in their great-grandchildren, 19% in their siblings, 14% in their half-siblings, and 9% in their siblings' children. The answer to the common man's problem is that the taint of 1087 schizophrenic patients will be represented in their grandchildren's generation by eight definite schizophrenias. The difference is, factually, primarily a matter of the low reproductivity and high mortality of schizophrenics; statistically, it seems to be a matter of attempting to estimate " how many secondary cases would have occurred among these individuals if they had all survived the danger-period for schizophrenia."

The secondary results, as indicated, include clear determinations of fertility (53% of the probands had no children, 77% no grandchildren; the whole proband group had 2000 children and 1016 grandchildren—in a precontraceptive age) and mortality (about half of all the children of probands and nearly a quarter of their grandchildren died before twenty). They also include: evidence of a preponderance of tuberculosis among the causes of death: indications that nonschizoform psychopathy, feeblemindedness, alcoholism, and criminality (except as a consequence of schizophrenia itself) are not associated with schizophrenia; and a conclusion, admittedly limited by the difficulties of field work on people long dead, that hereditary predisposition to schizophrenia was present in at least a third of the probands' parents, uncles, and aunts. Chapter V is devoted to protocols of secondary and special cases, and affords a sample of the diagnostic criteria in the more doubtful instances. Incidental

findings of interest are that, while there are practically no sex differences, the " nuclear " schizophrenias (catatonic and hebephrenic) behave in a sharply different manner from the " peripheral " group (paranoid and simplex), and that about 70% of what fertility there is is prepsychotic, so that only a very slight reduction, beyond that effected by nature, could be expected from segregation or sterilization.

It seems entirely reasonable on the basis of the evidence presented to believe that schizophrenia is partly conditioned by general tissue defects having germ-plasm determiners. (To the author this seemed obvious from the beginning, and as a result of his study he adds to this basic hypothesis that the transmission is Mendelian recessive and involves a single gene pair, and that the tissue affected is the reticulo-endothelial system.) Nevertheless, there are certain logical precautions which are regularly missing in medically conceived investigations of this sort, and which, in view of the importance of the problem and the growing maturity of the field, it does not seem excessive to suggest. The most important (and most difficult) of these is control against possible pathogenic effects of environment in early childhood. We have strongly suggestive evidence from several sources that the young child learns his emotional habits, as he learns other habits, from the only source available to him, namely, the adults with whom he lives, and it would be surprising if the emotional habit system called schizophrenia should prove an exception to this rule; at any rate, in an investigation designed to be precise it would do no harm to introduce this control. A further possible source of error is lay diagnosis, which, as in the case of subjects long dead as recalled by naïve or elderly persons with every conceivable emotional distortion, may be very crude indeed; it is at least doubtful whether the superficially impressive size of the notorious Jukes material, for example, has not been more than counterbalanced by its profound social effect in fixing in the minds of hundreds of thousands of students a conclusion which, whether true or not, is certainly of extremely low probability. A third difficulty is lack of control against possible cultural pathogenic pressures, and this may be looked upon as only one aspect of the general problem of normal controls. It is convenient, but hardly rigorous, to compare an experimental sample of this sort with some other investigator's supposedly normal sample drawn, perhaps, from another social class in a different time and place. At this stage of our knowledge it would be far better research strategy to match each proband with a known nonschizophrenic of the same sex, approximate age, socioeconomic status, and possibly family background, and then to investigate both by precisely the same techniques. Finally, while we have the customary respect for experienced and professional diagnostic competence, this purely affective support does not seem the most secure imaginable for conclusions of this weight. A psychologist observing the behavior of a child, or an examiner marking a test, does not rest content with assuring the reader of his report that he exercised the greatest possible care; rather, he presents the evidence, such as the percentage of agreement with another observer working independently, from which the reader may judge the reliability of his observations. A good deal of spadework in this region is probably propaedeutic to this

kind of investigation. In the first place, we need some sort of yardstick for the schizophrenic behavior continuum, analogous to a scale for the grading of handwriting; such a yardstick could probably be made up of units of a few hundred feet of sound-movie film, duly edited by a group of experts to represent whatever might be agreed upon as approximately equal increments of severity. Studies could then be made of the reliability of assignment of specific observations to positions on the scale, and observers could be trained to a desired criterion of expertness before being sent into the field to observe descendants of schizophrenics and their matched normal controls.

What we are suggesting, in brief, is increased statistical, *i.e.* logical, sophistication in the design of important studies of this sort. It is true that the size of the material examinable within given limitations of time and expense would be enormously reduced. What we seek, however, is increased probability of conclusions; and contemporary statistical theory has progressed beyond the point of ascribing this chiefly to increase of populations.

R. R. WILLOUGHBY.

Brown University.

LANDIS, C., & PAGE, J. D. Modern society and mental disease. New York: Farrar & Rinehart, 1938. Pp. xi+190.

If *multum in parvo* were ever appropriately applicable to any book it is to this one. It is in essence a huge handbook boiled down into a few pages, and a tremendous amount of labor must have gone into its production. For all this, workers in many fields will long be indebted to its authors. For information on any and every statistical aspect of mental disease there is not likely to be a better reference text for many years. Psychologists, sociologists, biologists, statisticians, geneticists, and eugenicists must all use this summary for convenience and for the last, the most up-to-date, word on many topics. There is material here for the teacher and for the researcher, and there is material here to make every scholarly person pause and consider. The authors have apparently ransacked the earth for their information. There was the searching of all available books and journal articles; government reports from more than a dozen countries were examined; and there was correspondence with many officials in many countries. For all of these sources a useful list appears in the Appendix.

One would have to reproduce the book to reproduce the findings of the authors. But, in all too imperfect summary, that most interesting to psychologists is their conclusion that the so-called psychogenic theory of etiology for the functional psychoses is without adequate foundation. The evidence, as summarized, points to an intrinsic or constitutional defect of some sort at their base. But this does not mean that these diseases are so clearly hereditary that a simple eugenic program would serve to stamp them out. Rather, it is made clear that in the light of our present knowledge (or ignorance) there is little justification for a wholesale eugenic program by the sterilization route.

Obviously, no teacher of abnormal phenomena and no investigator in the field can afford to ignore this volume. Most will find it an indispensable *vade mecum*.

EDMUND S. CONKLIN.

Indiana University.

GILBERT, M. S. Biography of the unborn. Baltimore: Williams & Wilkins, 1938. Pp. x+132.

As might be guessed, *Biography of the unborn,* written by an embryologist, is a description of the development of the human being from the time the minute ovum and microscopic sperm unite in the creation of a human life until the child emerges from the parent body.

While the story is narrated chronologically—a chapter for each month, the author has artfully and skillfully woven her tale so that one aspect of development is completed to the satisfaction of the reader before another aspect is begun. Such a simply told story of one of the most intricate processes of nature could only be written by a person who knows her subject matter thoroughly and who has also integrated the facts into an analyzed, but schematic, whole. The lucid exposition does not oversimplify the mechanics of development but, instead, serves to emphasize the complexity and marvel of life mechanisms.

Each of the ten chapters from " Genesis " to " Exodus " stresses the most characteristic aspect of development for the successive stages of growth, but continuity of exposition is maintained by recurrent reference to the various phases of morphogenesis. Finally, Chapter 11 discusses twinning and abnormalities of growth. A list of suggested readings and a glossary complete the book.

From a psychological point of view it is unfortunate to conclude with an emphasis on abnormalities, particularly since the book is written for the layman and will undoubtedly hold particular interest for the expectant mother. To read of harelips and one-eyed monsters can only serve to make her anxious. However, it is difficult, *a priori*, to judge the reactions of other nonscientific persons to this chapter. Instead of causing horror and anxiety, the explanation may make the abnormal appear so normal that marked physical defects lose some of their stigma. In any case, it would have been better, in my opinion, either to have completely omitted reference to growth abnormalities or to have ameliorated their discussion by including them at appropriate places in the description of normal growth. But it is unfair to be overcritical on this point when the work as a whole is of unquestioned value; it deserves high praise.

As a contribution to psychology, the book might well be required as underpinning for all students—particularly students of child psychology, because it presents undeniable and concrete evidence of the progressive patterning and repatterning that occurs in development, whether behavioral or physical. Students of general psychology are too unaware that it is only by examining the processes of behavior development that one can fully understand its mature expression; and students of child psychology are too likely to consider behavior development as merely

more skillful functioning. They fail to realize that just as three kidneys are formed and discarded in the growth processes of fetal life, so many child behaviors are formed, discarded, and built anew to meet the successive stage of growth.

Dr. Gilbert's book is a valuable contribution to the dissemination and humanizing of scientific phenomena.

HELEN THOMPSON.

Clinic of Child Development, Yale University.

CURTI, M. W. Child psychology. (2nd ed.) New York: Longmans, Green, 1938. Pp. vii+458.

In 1930 the first edition of Dr. Curti's *Child psychology* was published, and for two academic generations it has been a familiar text in many college courses. The present reviewer must frankly confess that the first edition has been to him just another book on child psychology—not a particularly good one, but still not very bad—just mediocre. With this judgment the second edition stands in vivid contrast. In the Preface of the new volume Dr. Curti states the hope that her psychological thinking has matured. The affirmative evidence that it has is repeatedly available in the text.

To those familiar with the first edition a formal appraisal will be significant. While the new book is only 2% longer in number of pages, a considerable change in typographic style has allowed a real increase in content of 13%. Even these figures do not fully represent the quantitative change. The chapter on juvenile delinquency, which had no real place in this book, has been eliminated. The chapter on play has been omitted as such, but certain pertinent material has been made part of a more general chapter on problems of mental life. The material on heredity has been rewritten so that pure biology is minimized and there is a real attempt at a critical appraisal of the parts played by heredity and environment in the child's development. Two chapters have been added in which the problems of individual differences and general intelligence are discussed. Following a rewritten "Introduction" there is a new second chapter which presents a condensed history of one child's development. This history serves to give the student a concrete picture of what the author is planning to discuss and is used throughout the text by way of illustration. Another valuable addition is a brief bibliographic note at the end of each chapter evaluating several books pertinent to the content of the chapter.

The contents of this edition systematically organize the picture of behavior development. A mere enumeration of chapter topics illustrates this. The biological organism—with due emphasis on hereditary, intra-uterine, maturational, and physiological factors—is discussed in three chapters which depict the newborn and what he has to build with. Next, the general problem of individual differences in various behavior functions, including general intelligence, is treated in two chapters. With the groundwork laid, the processes of behavior change are shown to take place by learning (the conditioned response, complex habits, growth of

meanings, including speech) until the complexities of human behavior are possible. The discussion of the more complex behavior includes reflective thought, social behavior, and, finally, the formation of personality. It is evident that this organization is much more logical than that of the first edition. Furthermore, the author has carefully reviewed every section of the old book, omitting or revising those parts which newer research has changed or made more clear, but retaining the old that is good. Nearly half of the references cited are to material published since 1930, the date of the first edition.

Perhaps the point to be most admired in the new text is the consistent, but not slavish, adherence to a point of view. Too many books on child psychology are either compendia of experimental abstracts, eclectic compilations interpreted in words of one syllable, or outright child study manuals written for a nonpsychological public. Dr. Curti is fundamentally a Chicago functionalist, and it is toward this theoretical position that she orients her work. However, she lives up to her own advice to students: "No matter how valuable a consistent point of view is, as an aid to thinking, it may become a hindrance if not held in the true spirit of science—critically, open-mindedly, humbly—as a tool for the more effective pursuit of truth." Systematically, this book is consistent without becoming fanatic; in contrast to the first edition it is a well-organized and carefully thought-out exposition of the development of child behavior.

C. M. Louttit.

Indiana University.

Harrower, M. R. The psychologist at work: an introduction to experimental psychology. New York: Harper, 1938. Pp. xiv+184.

If the title, "The Psychologist at Work," should mislead anyone into conceiving that this volume is concerned with the impure psychology, the contents of the early pages will soon disabuse him of the notion. He is told unequivocally (p. 3) that "psychology as a branch of learning has no concern with bettering the worldly or economic status of the individual." Having thus decapitated psychotechnology at a single stroke, Miss Harrower rubs salt into the wound by implying that the only psychology other than the 'academic' is that which attempts to "guarantee eloquence, personality, magnetism, charm, wealth, confidence, remarkable memory . . ." Thus vanish the pretty conceits and vain pretensions to sound methodology of Mayo, Viteles, Hull, Johnson, Strong, Lahy, Moede, Myers, Muscio, and their like, while the A.A.A.P. takes its proper place as an artisans' union. Miss Harrower is, alas, but following tradition when she suggests that there is and can be no 'branch of learning' concerned with the practical aspects of man's life. One wonders how much longer modern positivistic psychotechnology must knock at the temple gates before it gains admission as a research discipline worthy of a rear pew at great remove from the altar.

To return to our muttons, Miss Harrower has succeeded in writing an abridged account of psychology as she conceives it and, however

much one may question the catholicity of her definition, one may not question at all the dignity and insight with which she has treated the fields included in her definition. In 175 pages she manages to present stimulating and accurate descriptions of basic facts and basic methods in perception, in the genesis of behavior, in emotions, in remembering, and in learning. Her exposition shows clearly the impress of her connection with *Gestalt* psychology without, however, becoming at any point a plea for that point of view. Most of her references deal with publications of the last twenty years, but she is not averse to citing earlier researches.

The shortcomings of the volume (other than its conventional, but arbitrary, restriction of field) are less apparent than its virtues. When one deletes the physiological studies from her chapter on " The Emotional Side of Life," one is left with an impoverished fabric which probably represents the state of the field rather than a weakness in the author's treatment. Likewise, the attempts to ' explain' observed psychological facts by reference to a speculative neurology created by psychologists are scarcely original with the author. And her naïve faith that Ebbinghaus' nonsense syllables were really uniform and meaningless, while perhaps surprising in an exponent of *Gestalttheorie,* is shared with many other writers. There is, on the other hand, a pleasant absence of unguarded extrapolation from meager data and a praiseworthy abstinence from donning the long robe of Science which add no little to the dignity of the presentation.

All in all, the brief volume is a scholarly, but readable, affair which might well serve as corollary reading in many of the overroutinized elementary courses offered in this country today.

JOHN G. JENKINS.

University of Maryland.

ETHEL, G. Writing your novel. Portland: Scholastic Press, 1938. Pp. ix+273.

Artists and literary critics usually write about their subjects in terms that are vague and often supernatural. This essay, however, is written by a person who is well versed in the point of view and the terminology of contemporary objective psychology and who holds that literature must now stand consonant with modern science.

In scientific fashion the essay examines the novel as a phenomenon of human purposive behavior concerned with problems of adjustment. It proceeds, in stimulus-response terms, to examine character and its portrayal, setting, and action, and the technique of the novel in relation to its subject matter, purpose, and audience.

The consideration of absolute standards and judgments, art for art's sake, and romanticism strips the old arguments and postures of their confusion and glamour. They are laid bare in terms of their causes and consequences and are found as inadequate as illusion and wishful thinking. Art standards are the product of human behavior at a particular time in history and are, of course, relative human values. Because the artist is selective, and because his product has some purpose, it must

reflect his partisanship. But perhaps he is partisan for good reasons. His argument is his novel. He writes because he wants to influence the thinking and action of people in order that their lives may be fuller and richer. The road to more happy adjustment is through the realism of truth and not through fairy tales.

Although the style of the book may perhaps be ponderous, the content and approach signalize the extension of the view of modern psychology into literature.

RALPH H. GUNDLACH.

University of Washington.

BOOKS RECEIVED

BRUNTZ, G. G. Allied propaganda and the collapse of the German Empire in 1918. Stanford Univ.: Stanford Univ. Press, 1938. Pp. xiii+246.

BURLOUD, A. Principes d'une psychologie des tendances. Paris: Félix Alcan, 108, Boulevard Saint-Germain, VIᵉ, 1938. Pp. 430.

BUROS, O. K. (Ed.) The 1938 mental measurements yearbook of the School of Education, Rutgers University. New Brunswick: Rutgers Univ. Press, 1938. Pp. xiv+415.

DUNBAR, H. F. Emotions and bodily changes: a survey of literature on psychosomatic interrelationships 1910–1933. (2nd ed.) New York: Columbia Univ. Press, 1938. Pp. xl+601.

FLETCHER, P. Life without fear. New York: Dutton, 1939. Pp. 111.

FRITZ, M. F. Collection and presentation of statistical data in psychology and education. New York: Prentice-Hall, 1939. Pp. vi+58.

GEORGE, W. H. The scientist in action: a scientific study of his methods. New York: Emerson Books, 1938. Pp. 354.

KELLER, F. J., & VITELES, M. S. Vocational guidance throughout the world: a comparative survey. New York: Norton, 1937. Pp. xiii+575.

ROBERTS, H. The troubled mind: a general account of the human mind, and its disorders and their remedies. (With chapters on the Insanities by M. N. Jackson.) New York: Dutton, 1939. Pp. iv+284.

SANDIFORD, P. Foundations of educational psychology: Nature's gifts to man. New York: Longmans, Green, 1938. Pp. xv+464.

THURSTONE, L. L. Primary mental abilities. Chicago: Univ. Chicago Press, 1938. Pp. 121.

NOTES AND NEWS

Dr. George Van Ness Dearborn, chief of the Department of Medical Psychology of the U. S. Veterans Administration, has died at the age of sixty-nine years.—*Science*.

Dr. John W. McGarvey, instructor of psychology at Mount Holyoke College, South Hadley, Massachusetts, died on December 26.

Dr. Edward H. Cameron, professor of education at the University of Illinois and director of the University summer session, died on December 20 at the Albert Billings Hospital, Chicago, at the age of 63.

Dr. Karl Bühler of Vienna has accepted a temporary position (until June, 1940) at St. Scholastica in Duluth, and will assume a teaching position at Texas this coming summer.

Doctor Norman Cameron, formerly associate in psychiatry in the Johns Hopkins Medical School and resident psychiatrist in the Johns Hopkins Hospital, has resigned to take charge of the laboratory for experimental psychology and psychopathology at the Payne Whitney Clinic, New York Hospital. He has been appointed associate professor of psychology in the Cornell Medical College and assistant attending psychiatrist in the New York Hospital.

Dr. Henry N. Peters, assistant professor of psychology at the University of Missouri, has been granted a leave of absence from February to September, 1939, in order that he may serve as director of classification in the Missouri State Penitentiary at Jefferson City, Missouri. Mr. Willis H. McCann, recently at Indiana University, has been appointed instructor in psychology for the current semester.

A meeting of members of the American Association for Applied Psychology who had subscribed to the incorporation of the Association, granted under the laws of the State of Indiana on December 14, 1938, ratified the revised by-laws, and officially recognized the election of officers and members of the Association. This meeting was held at Ohio State University on December 21, 1938. Affiliation with the American Association for the Advancement of Science was officially endorsed by the members of the A.A.A.P. who were present, and affiliation with the American Psychological Association was ratified at the same time.

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A COURSE in the application of the Rorschach Method will be offered by Dr. S. J. Beck at Michael Reese Hospital, Chicago, Illinois, May 15–20, 1939 (the week following the annual convention of the American Psychiatric Association). Tentative plans are also being made for a similar 10-day course to be offered later in the summer, the dates being arranged to meet the convenience of prospective registrants. Information concerning these courses may be obtained from the Librarian, Michael Reese Hospital.

A NEW scientific periodical, *Psychosomatic Medicine,* started publication in January of this year. The journal will be published quarterly on a coöperative, nonprofit, nonsalary basis and will be sponsored by the National Research Council, Division of Anthropology and Psychology, Committee on Problems of Neurotic Behavior. The aim of the journal is to "encourage and bring together studies which make a contribution to the understanding of the organism as a whole, in somatic and psychic aspects." "Each number will include a review of relevant literature in the field of one or more of the medical and research specialties." In addition, *Monograph Supplements* will be published, and will include experimental data from longer studies. The Board of Editors invites submission of manuscripts for consideration for publication in these journals. Reprints of all articles published will be mailed to authors of those articles before distribution of the journal. It is expected that all authors will agree to the purchase of at least 200 preprints. Correspondence relative to the publication of articles should be addressed to Dr. Flanders Dunbar, Managing Editor, Academy of Medicine Building, Room 445, 2 East 103rd Street, New York City.

THE Public Information Section of the Works Progress Administration, 70 Columbus Avenue, New York City, has prepared a news release of facts revealed by the Psychological Index Project of the Administration of that city. These facts follow in summary:

World interest in psychological research, as measured by the number of articles published in scientific journals, continued at a high pitch through 1938. Articles published in all languages totaled 6693—an increase of more than 10% over 1937. Only one year in the post-War period—1931—topped this figure with 6792 articles.

An analysis of the distribution by languages discloses that maintenance of a high level of activity in English is the principal reason for continuance of the world total at close to peak figures. A very high percentage of the titles published in English is American. Titles in English reached a total of 4038 in 1938—an increase of more than 7% over 1937 and a new peak for the period since the War. Unusual features continue to be presented by the trend of publications in German. While the total there reached 1478 in 1938—an increase of more than 27% over 1933, it is still more than 44% below the post-War zenith of 2658 articles published in 1930. Titles in French, numbering 413, indicate a 9% increase over 1937. The analysis reveals that the rate of increase in all languages from

year to year has been subsiding recently from the pace set during the decade of recovery following the War.

The WPA study is based, during the earlier part of the period under consideration, upon tabulations made from listings in the *Psychological Index* by Professor Samuel W. Fernberger of the University of Pennsylvania. The latter part of the period is based upon a similar count in the *Psychological Abstracts* made within the Psychological Index Project. Professor Albert T. Poffenberger of Columbia University is technical director and Harold C. Brown is supervisor of this project.

DR. C. M. LOUTTIT, Indiana University, Bloomington, Indiana, who is attempting to compile a directory of psychological associations in the United States, would appreciate having the secretaries of such associations, especially those of a local nature, send information about their organizations to him. The information desired includes name, purposes, officers, size of membership, and frequency and dates of meetings.

THE prize of $1000, which is awarded each year by the American Association for the Advancement of Science for an outstanding contribution to science, was awarded this year at the meeting of the Association in Richmond, Virginia, to Dr. Norman R. F. Maier, assistant professor of psychology in the University of Michigan, for his paper on " Experimentally Produced Neurotic Behavior in the Rat." In referring to Dr. Maier's experiments, the committee on award stated: " He has produced in rats behavior, the neurotic character of which seems to the committee, and to the critical audience which heard his paper, to be beyond doubt. This behavior was produced under conditions so controlled that cause and effect can be scientifically analyzed. The committee does not feel that the author's analysis of the phenomena is complete, nor does it believe that the small number of rats in which neurotic behavior was experimentally induced is sufficient for generalization or sweeping conclusions, and it gives credit to Dr. Maier for the conservatism he exhibited and for the scrupulous avoidance of applying his discoveries prematurely to the field in which they ultimately will be vastly significant—namely, neurotic behavior in human beings."—*Science*, 1939, **89**, No. 2301, 93.